

EMPIRICAL TESTING AND THEORY VALIDATION:

A STRUCTURAL EXPLANATION OF PERSISTENT  
CONTROVERSY IN POSITIVE ECONOMICS

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Ph.D.

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1978

To my father

## ACKNOWLEDGEMENTS

I am grateful to Professor P. Vandome for his diligent and critical supervision throughout the course of this research. Without his continuous encouragement this thesis would not have been possible. I would also like to thank Mr. R. Bhaskar for his comments on the philosophical part of the thesis.

I am indebted to the help I received from friends and colleagues in the University of Edinburgh, who read parts of the thesis, and to the critical discussions that ensued. Needless to mention that all remaining errors are entirely my own responsibility.

I would also like to thank Mrs. G. Young for giving me the benefit of her intelligence and competence in typing this thesis.

My gratitude also goes to my parents, and to my brother and his family, for their assistance and support. And last, but not least, I thank my wife for her endurance and patience.

## A B S T R A C T

Choosing between alternative economic theories has been a difficult task throughout the history of economic thought. An attempt is made in this thesis to offer an explanation of persistent conflict within Positive economics.

In the first part, the validational procedure in Positive economics is examined, and the place of the F-Twist controversy within it is described. In an effort to define terms used in this controversy, an ambiguity is found that permeates the structure of terms such as, assumptions, fundamental postulates, etc. In examining the criterion of objectivity in Positive economics, namely empirical testing, more ambiguities are found stemming from the irresolution of the Problem of Induction. These ambiguities are related to the positivist distinction between theory and fact and to the elusive justification of inductive inference. Finally, the reliability and objectivity of empirical evidence is put in question.

In the second part, the Monetary controversy is outlined. Initially the scene is set by accounting for the development of the controversy within the context of the policy developments during the post-war period. Then, the theoretical framework of the controversy is delineated and it is found that Monetarists and Fiscalists belong to the same theoretical framework differing only in the emphases they put within it. Finally, the empirical evidence by either side is examined and the conclusion is reached that it has proved indecisive for choosing between Monetarism and Fiscalism.

In the last part, critiques of Positive economics are reviewed, and various explanations of conflict in economics are discussed. It is found that in most of these explanations, causal relations are looked for, which attribute conflict to isolated factors stemming from the defects of economics. An alternative account of conflict in economics is propounded in which the logical 'matrix' behind persistent controversy is put forward. In this account a diagram is drawn structurally tracing and logically connecting opposing tendencies in Positive economics. Along with the logical structure a historical framework is also given within which the connecting relations are realized. The argument is put forth that, as far as the F-Twist and Monetary controversies are concerned, conflict in Positive economics cannot be resolved by empirical testing, due to structured ambiguities and contradictions in it.

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## I N T R O D U C T I O N

"This double differentiated existence must develop into a difference, and the difference into antithesis and contradiction"

K. Marx

## INTRODUCTION

The object of this thesis is to understand the logic of controversy within Positive economics. Yet controversy over alternative interpretations of the phenomena studied is not a characteristic peculiar to economics. Most sciences exhibit the same proliferation of competing theories. What is, however, peculiar to economics - and perhaps to all social sciences - is the tendency for controversies to remain persistently unresolved. One simply has to look to controversies such as those between Historicists versus Marginalists or Institutionalists versus Positivists, or to policy controversies such as those over money and prices, or to theoretical controversies such as those over capital, investment, consumption, perfect and imperfect competition, to see that they have remained largely unresolved. Although their intensity has fluctuated, controversies such as the above have appeared in various forms and shapes throughout the history of economics. For instance the Marginalists v. Historicists controversy reappeared in the form of the F-Twist controversy, or policy controversies that raged during Ricardo's time have re-emerged and have taken the shape of the Monetarist controversy. Whereas in the physical sciences one may find indisputable criteria for choosing between alternative theories, in economics there is even unresolved conflict for these criteria. According to Clarkson, "The history of economic thought is in part a history of a dispute over the criteria to be employed when adjudging the validity of contending economic theories."<sup>1</sup>. Although one might find it urgent to study economic phenomena such as capital or inflation, one encounters this phenomenon

in economics whereby no irrevocable criteria can be established for choosing between a Cambridge Massachussets and a Cambridge England interpretation of capital, or between a fiscalist and a monetarist interpretation of inflation.

Perhaps one might argue that controversy means progress, and thus forms part of the scientific process<sup>2</sup>. Yet, if compared with controversies in the 'harder' sciences - where perhaps controversy may be associated with progress - controversies in economics exhibit a permanence and stubborn persistence that has become a major characteristic of all the 'softer' sciences. As Myrdal says "In economics, on the contrary, (with natural sciences) all doctrines live on persistently"<sup>3</sup>. Thus, one can speak of controversy as a scientific tool bringing new knowledge, once there has been resolution and not continuous theoretical stalemate. Scientific progress is the result of resolved controversies, and not the outcome of perpetually conflicting theoretical tendencies<sup>4</sup>. Alternative schools of thought develop within and without the orthodox Positivist-Neoclassical framework creating persistent conflict. Each school professes to be the only holder of the key to the truth, and yet no truth is established. It may seem clear why controversies such as, for example, between Neoclassical and Marxian economics remain unsolved, since they pertain to two incompatible and alternative paradigms. Different languages, fundamental assumptions and methodologies account for the irresolution. However, it is not at all clear why controversies within Positive economics remain unresolved.

In contrast to inter-paradigmatic conflicts, participants in conflicts within Positive economics are agreed on how to resolve disagreements. The common methodology stipulates that the empirical testing of theories will sift the true from the false. According to Lipsey "disagreements over positive statements are appropriately settled by an appeal to facts"<sup>5</sup>. And indeed when controversy rages in Positive economics empirical testing comes to the fore. In controversies such as, for example, the investment function and Monetary controversies, empirical evidence has poured thick and fast. To no avail however. Despite the accumulation of empirical evidence the facts have shown favour to all sides. The irresolution of disagreements among economists has become, according to Myrdal, "proverbial"<sup>6</sup>. The failure of Positive economics to resolve major conflicts has been pointed out frequently by critics. For example Katouzian argues that "The failure of Positive economics . . . is to be seen particularly in the fact that no major economic hypothesis has yet been successfully refuted"<sup>7</sup>.

The paradox is however, that despite the continuous disagreement and the failure of empirical testing to decide between competing theories, Positive economists are still faithful to the doctrine that empirical testing will resolve the conflicts. "Yet we have faith" says Bronfenbrenner "that most if not all such positive disagreements will eventually be resolved"<sup>8</sup>. It should have been made clear to Positive economists by now that, according to their own methodology<sup>9</sup>, every hypothesis when confronted with counter-evidence should be rejected. And yet empirical testing has been a methodological

hypothesis which has not been rejected although it has been tested and contradicted. An attempt is made in this thesis to understand this binding faith towards the methodology of Positive economics.

It is indicative of crisis in a discipline when methodological discussion becomes relatively important<sup>10</sup>. Although most economists would disapprove of discussions on methodology<sup>11</sup> they would, with few exceptions, indulge in them with the excuse that "Methodological discussion, like calisthenics and spinach, is good for us"<sup>12</sup>. It seems that doubts about one's own philosophical principles have become a staple diet of the economists who constantly feel the stability of their paradigm shaken. As Kuhn notes "Only when they must choose between competing theories do scientists behave like philosophers"<sup>13</sup>. Thus, if this principle is correct then crisis has become a permanent feature of economics, since methodological discussion has been in the limelight from the times of Ricardo, Marx and Veblen to Friedman, Machlup and Samuelson. Given the permanent theoretical competition, it seems that there is always a need for the economist to turn into philosopher.

But where does methodological discussion lead us? Does it help the economist to resolve the crisis or make a rational (i.e. reasonable) choice of contending philosophical and theoretical positions? From what appears from the history of methodological discussion in economics the answer is negative. Despite the number of articles and books written on economic methodology, economists still disagree, and still cling to their own 'correct' methodology while the crisis

continues. Methodology, Samuelson observes "is a field where . . . every economist feels his ideas are as good as anyone else's"<sup>14</sup>. Surely it is a contradiction when a discipline that emphasizes rationality as its fundamental assumption is found in such an irrational position. It is indeed prima facie irrational to have unresolved conflict among 'rational' Positive economists, who are supposed to share the same methodology. Thus, it is important to the understanding of methodological conflict to attempt to unravel the logic behind this 'irrationality'. In so doing one comes closer to the fundamental contradictions in Positive economics and to the explanation of controversy. It is therefore part of the purpose of the thesis to examine this logic behind methodological conflict in Positive economics and relate it to a particular economic controversy.

Although methodology has been a widely discussed topic in economics, the subject of unresolved controversy has been relatively very little studied. Besides Krupp and Myrdal who have dealt with it somewhat explicitly<sup>15</sup>, most students of economic methodology tend to leave it in the background. The usual explanation given for persistent theoretical conflict relates to the ineffectiveness of Positive economics to overcome inherent value judgements<sup>16</sup>. Other explanations relate to the nature of the socio-economic process which eludes any kind of stable methodological procedure for the choice between alternative theories<sup>17</sup>. Still other explanations of disagreement in economics come from people who argue that economics is made up of theories that are not falsifiable, and that therefore no belief can be refuted decisively<sup>18</sup>. More recent explanations, taken after Kuhn,

relate to the paradigmatic character of Positive economics, as a factor deterring "empirical arguments . . . in shooting down strongly held beliefs"<sup>19</sup>. In my opinion, however, these explanations offer only a partial account of controversy, since they do not view the continuation of conflict as a necessary outcome of the structure of Positive economics, but rather as an outcome of its defects. For example, Myrdal's explanation in terms of implicit value judgements that hinder the resolution of controversy, or Krupp's explanation in terms of elastic ceteris paribus clauses and different initial strategies (see chapter 6), or Morgenstern's in terms of inaccurate data<sup>20</sup>, or, finally, Clarkson's in terms of the gap between abstract economic theory and concrete empirical reality<sup>21</sup>, are explanations that refer to defects of Positive economics. Although these explanations may be valid in themselves, they are nonetheless only isolated observations of a very complex problem. They do not cover a wide space of the logical ramifications behind unresolved dispute in economics. In contrast, by uncovering, as Samuels says, "the intellectual matrix of the conflicts"<sup>22</sup>, I feel that a better understanding of the logic of controversy is achieved. Instead of trying to find the immediate causes of conflict in economics, an attempt is made in this thesis to unravel the combinations and permutations of the ideas that form the structure supporting persistent controversy. Thus, strictly speaking, no explanation (in the sense of causation) is given here, but rather a description of the logical (necessary) and structural connections behind irreconcilable conflict.

As far as Positive economists are concerned, most of them brush away the problem of disagreement by claiming that irreconcilable conflict concerns questions about value judgements about which "men can ultimately only fight"<sup>23</sup>, whereas conflict that concerns positive judgements is in principle resolvable<sup>24</sup>. "Much of the disagreement" argues Bronfenbrenner, "is inevitable since it centers around economic values and policy recommendations and involves normative rather than positive economics"<sup>25</sup>. However, it is difficult for Positive economists to argue that the testing of the investment function or the evidence from either large-scale models or single-equation tests regarding the Monetary controversy, concerns value judgements (at least explicitly). If this were so then what would separate positive from normative judgements?

I feel it is important to bring the issue of persistent controversy to the foreground for two main reasons: firstly, because it sheds light on the whole structure of Positive economics and its assumptions, and thus leads to a better understanding, and therefore critique, of it. And secondly because it allows a synthesis of philosophic and economic arguments in a way that renders them inseparable. The inseparability of economic and methodological arguments is important because of the proliferation of economic controversies and the lack of established criteria for the choice between conflicting theories. One cannot speak purely of an economic issue without considering the alternative existing interpretations<sup>26</sup>. And if this is true, then automatically choice criteria have to be formed, which leads directly to methodology<sup>27</sup>. Yet, most economists would tend to take methodological

issues independently of economic issues. However, even if methodology is not explicit, tacit criteria are in fact applied for choosing one version of an economic issue over another. Given, therefore, permanent competition of theories within and without Positive economics on almost any issue, it seems worthwhile to try to understand persistent conflict in economics. This puts the validational procedure of Positive economics in a light that exposes the nature of both the methodology and ontology of economics. By studying the structural patterns that are behind conflict in economics, both methodological and ontological factors arise. In this way the two levels are put in a dialectical context. Importance is attached to one factor insofar as it is structurally related to the other. They cannot be seen separately, because both are defined only in relation to each other. As Lévi-Strauss says "The methodological integration of essence and form reflects, in its own way, a more necessary integration - that between method and reality."<sup>28</sup>.

In attempting an explanation of persistent conflict in economics I have used two sources: philosophy<sup>29</sup> and Structuralism<sup>30</sup>. The first one helped me to identify the factors that constitute the explanation, and the second helped me relate them. However I did not intend to be either philosophically complete or structurally adequate. Both sources were used in as much as they could contribute to the study of the phenomenon. As the mathematical economist or the econometrician borrows from mathematics or statistics to use in the study of economic phenomena, so have I borrowed from philosophy and Structuralism to use in the study of phenomena in economics.

The strategy with which I have decided to tackle the problem, and which is also reflected in the structure of the thesis, is to first describe the object of the research, which is the methodology of Positive economics, then show an inconsistency in it, i.e. a major unresolved conflict, constituting the phenomenon under study, and lastly offer an interpretation of the logic behind the persistence of the inconsistency.

In following this strategy I have separated the thesis into three parts. In Part I, Chapter 1, I discuss the process of theory validation in Positive economics and bring into the picture the issues involved in the F-Twist controversy. The major conclusion that stems from this chapter is that the definition and structure of fundamental assumptions as used by Positive economists is ambiguous, allowing polar interpretations of their function. In Chapter 2 I examine the nature of the objectivity criterion for theory selection in Positive economics, which is empirical evidence. In doing this I first discuss in general the justification of this criterion, and its relation to theory, and then in particular within the context of Positive economics. Then I juxtapose this criterion to an objectivity criterion from an alternative methodology, i.e. Marxism, in order to show the non-universality of its application. Lastly I consider the nature of this criterion and I conclude that besides being unreliable as an accurate reflection of economic reality, it is also ambiguous as used by Positive economists. Furthermore I add that this ambiguity stems from the impasse facing the economist when attempting to rationally justify it.

In Part II I examine the Monetary controversy and its irresolution. Chapter 3 gives an idea of the development of the controversy and its dialectical relation to policy successes and failures. From this I make the observation that at the policy level both sides have found favour and disfavour. Chapter 4 attempts to put the controversy into theoretical perspective and examine the issues involved. From this examination I conclude that both sides belong to the general neoclassical framework, differing only in degree and emphasis. In Chapter 5 I present the evidence and the empirical tests that have been used to support (or reject) either the monetarist or the fiscalist position. From this presentation I conclude that despite the issues being 'non-normative' in character and despite the enormous amount of empirical evidence accumulated during the controversy, the conflict still goes on with no decisive step taken towards either direction.

This conclusion in turn leads me to Part III in which I present an explanation of the persistence of the Monetary and F-Twist controversies. First, in Chapter 6, I make a critical review of some of the critiques levelled against Positive economics and also discuss some of the arguments used as explanations of conflict in economics. From this I find that although these arguments are sufficient they are not, however, necessary explanations as they do not cover the whole of the logic of conflict. I also make the observation that although most of the critics are correct as far as Positive economics is concerned, nevertheless as soon as they attempt to offer an alternative to it they are ambiguous. Finally, the last chapter puts forward an alternative explanation, by reconstructing

step by step the logic and structure of Positive economics. In doing this, certain observations and a diagram emerge depicting the mechanism that nurtures and necessitates persistent conflict in Positive economics.

I hope the reader will forgive the repetition of certain obvious points. As I hope will become apparent while reading the thesis, the repetition is necessitated by the nature of the explanation which I am putting forward. Each component part of the explanation is taken out of the structure of Positive economics and under different circumstances is given a different meaning. For instance, the positivist dichotomy between analytic and synthetic statements is analysed either in the light of the Problems of Knowledge, or in its economic application, or in its function in creating controversies in economics. In all cases the dichotomy and its meaning have to be repeated so as to provide the basis for the explanation. Another instance is the repetition of the point about the Problems of knowledge. The point is so obvious, even to non-philosophers, that it needs little mention, let alone repetition. However, the implications stemming from these Problems are not brought to bear critically upon Positive economics, but to furnish a context within which the phenomenon of persistent controversy may be explained.

## INTRODUCTION:

## FOOTNOTES

1. P.E. Clarkson, The Theory of Consumer Demand: A Critical Appraisal, 1963, p.3. Or according to Samuels "The history of the discipline is the history of controversy . . . very often the controversies have been continuing and/or replicated . . . many twentieth century controversies are but modern versions . . . of nineteenth century disputes." "The History of Economic Thought as Intellectual History", History of Political Economy 1974, p.314. Also as Knight comments "Methodological controversies . . . are so characteristic of the social sciences, in contrast with those dealing with nature", Institutionalism and Empiricism in Economics", American Economic Review, 1952, p.53.
2. See, e.g. S. Krupp, "Types of Controversy in Economics" in his The Structure of Economic Science, 1966, pp.39-40.
3. G. Myrdal, "How Scientific Are the Social Sciences", Economies et Societes, 1972, p.1489. See also J.M. Culbertson, Macroeconomic Theory and Stabilization Policy, 1971, p.113 and pp.114-115.
4. Ibid., p.1489.
5. R.G. Lipsey, An Introduction to Positive Economics, 1963, p.4. See also M. Friedman who says that "The difference [between policy controversies] is not a moral one but a scientific one, in principle capable of being resolved by empirical evidence", "Value Judgements in Economics" in S. Hook's Human Values and Economic Policy, 1967, p.87.
6. The Political Element in the Development of Economic Theory, 1953, p.XIII, and "How Scientific Are . . ." op.cit., p.1489.
7. M.A. Katouzian, "Scientific Method and Positive Economics", Scottish Journal of Political Economy, 1974, p.282. Also Rotwein claims that "in . . . economics . . . many hypotheses . . . continue, and after decades of disagreement, . . . remain subjects of heated controversy" in "Mathematical Economics: The Empirical View and an Appeal for Pluralism" in Krupp's "The Structure of Economic Science", op.cit., pp.110-111. See also A. Lowe, "Toward a Science of Political Economics" in R. Heilbroner's Economic Means and Social Ends, 1969, p.3, and T.W. Hutchison, 'Positive' Economics and Policy Objectives, 1964, p.18. Culbertson also comments that "competing research studies applying the standard methodology reach conflicting conclusions, support theories with quite different implications for substantive interpretation and policy information", Macroeconomic Theory and Stabilization Policy, op.cit., p.82.

## FOOTNOTES (cont.)

8. "A 'Middlebrow' Introduction to Economic Methodology" in Krupp's "The Structure of Economic Science", op.cit., p.13. This, however, contrasts with Culbertson's remark that "There seems to be no basis for believing that continued application of this methodology will lead to convergence toward a valid theory." "Macroeconomic Theory . . ." op.cit., p.102.
9. Following Machlup I shall distinguish between method and methodology. I consider the term 'method' to apply to a set of techniques, such as game theory, and the term 'methodology' to apply to the epistemological assumptions of a discipline. Thus a given method may imply a set of methodological assumptions but not vice versa. See F. Machlup, "Problems of Methodology" in the American Economic Review Papers and Proceedings, 1963, p.204.
10. See T.S. Kuhn, The Structure of Scientific Revolutions, 2nd Edition, 1962, p.88. See also B. Ward, What's Wrong with Economics, 1972, pp.32-33.
11. See for example, A. Lerner, "Prof. Samuelson on Theory and Realism: Comment", American Economic Review, 1965, p.1153, and R. Harrod, "The Scope and Method of Economics", Economic Journal, 1938, p.383. As Clarkson also comments "economists almost invariably disapprove of methodological discussions", "The Theory of Consumer Demand"; op.cit., p.6.
12. P.A. Samuelson, "Discussion" in "Problems of Methodology", American Economic Review Papers and Proceedings, 1963, p.231.
13. T.S. Kuhn, "Logic of Discovery or Psychology of Research", in I. Lakatos and A. Musgrave's Criticism and the Growth of Knowledge, 1970, p.7.
14. "Prof. Samuelson on Theory and Realism", American Economic Review, 1965, p.1164. See also Culbertson, "Macroeconomic Theory . . .", op.cit., p.115.
15. Krupp's and Myrdal's critique of Positive economics, and their explanation of controversy will be discussed in chapter 6.
16. See, e.g. Myrdal, "The Political Element in the Development of Economic Theory", op.cit., p.19.
17. See, e.g. F.H. Knight, On the History and Method of Economics, 1956, pp.154-5.
18. See, e.g. K. Klapholz and J. Agassi, "Methodological Prescriptions in Economics", Economica, 1959, pp.60-74.
19. B. Ward, "What's Wrong with Economics", op.cit., p.174. See also p.176.
20. O. Morgenstern, On the Accuracy of Economic Observations, 1950, p.50.

## FOOTNOTES (cont.)

21. P.E. Clarkson, "The Theory of Consumer Demand", op.cit., p.5.
22. Samuels, "The History of Economics . . ." op.cit., p.314.
23. M. Friedman, "The Methodology of Positive Economics" in his Essays in Positive Economics, 1953, p.5.
24. Friedman, "Value Judgements . . .", op.cit., p.87.
25. Bronfenbrenner, "A 'Middlebrow' Introduction . . .", op.cit., p.13.
26. See Samuels, "The History of Economics . . .", op.cit., p.314.
27. "Our justification of ourselves is basically a methodological one . . . to define 'our best posit', we must discriminate among methodologies involving substantially different courses of development of knowledge and different responsiveness of knowledge to ideology and special interest", Culbertson, "Macroeconomic Theory . . .", op.cit., pp.114-5.
28. C. Lévi-Strauss, "Totemism", 1962, p.154.
29. Although the philosophical ideas presented in my thesis show some affinity to the ideas presented in Hollis and Nell's "Rational Economic Man" (1975), this affinity is superficial. Whereas in the case of Hollis and Nell these ideas have been used strictly as a critique against Positive economics, in my case they have been used as a framework with which to identify the factors that contribute to the explanation of the methodological impasse facing Positive economics. (For comments on Hollis and Nell's critique see the Appendix "Rational Scientific Man" at the end of the thesis.) Although I gratefully acknowledge the inspiration that I drew from Hollis and Nell's book, I must point out that their book was published while my research was in progress and most of the ideas developed in this thesis were already in my mind.
30. By Structuralism I mean the method used mainly by anthropologists to delineate social phenomena. I have confined analysis however to the brand of Structuralism used by E. Leach, as the Structuralism used by C. Lévi-Strauss is restricted only to primitive cultures. Leach's modified Structuralism allows the application of the method to any cultural phenomenon, (see, e.g. E. Leach, Lévi-Strauss, 1970, Michelangelo's Genesis, Times Literary Supplement, March 18, 1977, and Genesis as Myth, 1969, and Lévi-Strauss, "Totemism", op.cit., esp. the introduction by R. Poole). Of course, I have not followed any exact pattern of Structuralism, but I have only used hints and ideas that helped me shape the form of my explanation. I believe Structuralism to be a more advantageous (although complementary) method to study conflict in economics than, say, sociology of knowledge, as it fits the polar (binary) and dialectic structure of the phenomenon in question.

P A R T      I

P O S I T I V E    E C O N O M I C S

"You want something with a tendency, a selection of facts with a well-defined tendency . . ."

"Not at all. There's no question of selecting anything of a tendentious nature. We don't want any bias. Complete impartiality, that must be our only tendency."

Dostoyevsky, "The Possessed".

## P A R T    I

## P O S I T I V E    E C O N O M I C S

## O U T L I N E

The purpose of this part is to fulfil the first requirement of the strategy delineated in the introduction of the thesis, that is to describe the object of this investigation. In other words its objective is to make an exposition of the methodology of Positive economics and examine the role and status of empirical evidence in it.

In doing this Part I will be separated into two chapters. The first chapter will outline the process of theory selection in Positive economics and define the role of empirical testing as an objectivity criterion. In addition it will attempt to show that the F-Twist controversy, a controversy concerning the priority of testing predictions rather than assumptions, is a conflict based on an ambiguity which is structurally embedded within the methodological paradigm of Positive economics.

The second chapter will examine the nature of empirical evidence. In order to set the scene for this examination a general account of the nature of empirical evidence and the Problem of Inductive inference will be presented. Following this a sketch of what counts as legitimate 'facts' for Positive economics will be drawn and the method and problems of constructing them will be outlined. This chapter will also briefly describe some methodological assumptions in Marxism and try to relate the status of empirical evidence in them. In doing this it is purported to show that the emphasis on empirical

testing is an epistemological datum contingent to Positive economics, and as such cannot claim universal objectivity. In order to do this a different, and alternative, methodological paradigm has to be outlined. By showing that an alternative and valid set of objectivity criteria exist, the status of empirical testing will be attributed its appropriate place, which is within the methodological walls of Positive economics. Finally, in this chapter, various problems concerning the validity, accuracy and reliability of empirical evidence will be indicated.

At the outset the reader should be warned that what follows is not a survey of writings and criticisms about the methodology of Positive economics. It is rather an impression of what Positive economics is all about as expounded in the writings of Positive economists within the context of the F-Twist controversy. Furthermore, the objective is not to criticize Positive economics as this task has been successfully achieved by numerous critics<sup>1</sup>. My objective here is rather to describe Positive economics as one methodological paradigm among others, outline its assumptions and discover the ambiguities that will form part of the explanation of persistent conflict in economics.

CHAPTER 1

T H E   P R O C E S S   O F   T H E O R Y

V A L I D A T I O N   I N

P O S I T I V E   E C O N O M I C S

## CHAPTER 1:

THE PROCESS OF THEORY VALIDATION  
IN POSITIVE ECONOMICS

A. EXPOSITION OF THE METHODOLOGY OF POSITIVE ECONOMICS  
a. 'WHAT IS' ECONOMICS

The term 'positive', as Positive economists use it in the context of scientific inquiry, refers to propositions that deal with events in the world as they actually are. That is, "positive science may be defined as a body of systematized knowledge concerning what is"<sup>2</sup>. The term 'positive' is employed in order to separate objective economic knowledge from its normative, instrumental and practical implications, and avoid any confusion and conflict stemming from the possible overlapping of these distinctive branches of inquiry<sup>3</sup>.

Although the above definition of 'positive' appears to be clear enough, it seems necessary to try to elaborate it as it does not really ascribe any particular meaning to the term, but rather it re-describes it in terms of the notion 'what is'. By 'what is' Positive economists mean a set of propositions that describe events as they actually occur. That is, as events are perceived by the economist occurring objectively and free from bias. 'What is' denotes that the observer can appeal to a reality that is outside his/her subjective reality. It emphasizes the fact that phenomena or events may be accounted in a 'value-free' manner. However, it is recognised by Positive economists that although bias does enter in positive research<sup>4</sup>, this bias is ultimately purged by the objectivity of empirical observations. Thus, it is empirical evidence that renders a proposition ultimately 'positive'.

'What is' thus implies that there is a separate reality from the reality of the observer<sup>5</sup>. It is a 'reality', in the form of relations among economic variables, that is always 'there', as a physical reality, ready to be studied, observed, quantified and ultimately controlled. The notion of 'what is' implies a self-disciplined scientist who always sifts out the metaphysical elements from his/her scientific observations.

In reference to this type of external objectivity, and in anticipation of chapters 6 and 7, it is worth noting that the obvious objections raised against such contentions, as for example, the objections that the dividing line between the observer and the observed is blurred<sup>6</sup>, or that economic variables, because they involve human beings, cannot be controlled and therefore cannot be scientifically assessed in the laboratory sense<sup>7</sup>, or that observers of socio-economic phenomena use value-loaded terminology<sup>8</sup>, or that ideological bias creeps in through the class orientation of the observer<sup>9</sup>, can be countered by some of the arguments used for safeguarding objectivity in Positive economics<sup>10</sup>.

The positivist methodological framework rescues the Positive economist from this type of objections. For instance the first objection, i.e. the blurred relationship between the observer and the observed, is countered by the argument to the effect that Positive economics confines itself to the measurable and therefore to the objective<sup>11</sup>. Even though the observer might be subjectively biased,

the appeal to quantified evidence demystifies his/her conjectures<sup>12</sup> (the notorious difficulty of quantifying economic reality is, however, duly acknowledged, but it is not considered a problem in principle different from the ones facing a physicist<sup>13</sup>).

The second objection, i.e. that economic phenomena consist of unpredictable human beings, falls against the argument that Positive economics does not deal with the bearers of the variables per se, but only with the relations between them. As in physics so in economics it is not the particles that are in question but their behaviour and the complex pattern of relations stemming from it. The task of the Positive economist is to formulate hypotheses about these relations. The rationale behind this argument is that although human beings are whimsical and uncontrollable, their behaviour can be contained statistically, very much like the random behaviour (movement) of particles in a container can be determined statistically<sup>14</sup>.

The third objection, i.e. that of using value-laden terminology, is usually countered with a corollary of the argument of measurability. It is contended that the testing and falsification of economic propositions with empirical, quantified evidence, will ultimately reduce the value-laden terms used in the propositions into positive ones. The use of such terms as equilibrium or perfect competition are attributed a linguistic-analytical function which helps order complex observations<sup>15</sup>. Their significance is measured by the success they have in providing empirically meaningful theories or predictions that can be tested with quantified facts<sup>16</sup>.

Finally, the fourth objection, i.e. the class bias of the observer, can be countered with the positivist claim that the independence of the methodology used, i.e. testing theories with empirical evidence, will provide the criterion which will separate the ideological from the 'real'. Again it is empirical facts, acting as arbiters of possible ideological grievance, that play the role of the neutral, objective, and above suspicion criterion of 'truthfulness' or 'reality'<sup>17</sup>.

Setting aside for the moment other criticisms of Positive economics, to be discussed at a later stage along with more detailed analysis of the above objections, I can proceed to further examine the meaning of 'what is' economic propositions.

Statements concerning 'what is' are distinguished from statements concerning 'what ought to be'. The latter by definition refer to value judgements. That is, they deal explicitly with choice between objectives in contrast to the former which refer only to the 'possible' (the actual) and not to the 'desirable': "Political economy . . . furnishes information as to the probable consequences of given lines of action, but does not [itself] pass moral judgements, or pronounce what ought or what ought not to be"<sup>18</sup>. According to this view the economist is likened to the physicist who investigates the atom but does not pass moral judgements as to the desirability of atom bombs or the use of nuclear energy. Equivalently, the economist studies the structure of the economy as 'it really' is and does not get involved with the practical question of whether this structure is

desirable or not<sup>19</sup>. However, once this kind of explicit valuation is taken into account, problems arise when one takes into consideration the existence of implicit valuations. By implicit valuations I mean values that are not explicitly stated in terms of choices between given objectives, but rather values that stem from largely unconscious bias which influences the direction of one's scientific research<sup>20</sup>.

The major characteristic that sharply distinguishes positive from normative statements is that "pure value-judgements cannot be tested by empirical procedures and therefore cannot be admitted into the body of Positive science"<sup>21</sup>, whereas "Positive economics . . . deals with statements that could conceivably be shown to be wrong (i.e., falsified) by actual observations of the world"<sup>22</sup>. The dichotomy is drawn thus: between statements that appeal to tastes and subjective valuations and statements whose truth or falsity can be established by an appeal to empirical evidence. Thus, 'positive' is defined in terms of propositions that deal with 'actual' as distinct to 'desirable' events, or with consequences of given actions, the objectivity of which is determined by 'economic reality' as represented by quantified empirical evidence.

Another implication of the term 'positive' refers to the possibility of observing social phenomena in more or less the same manner as the 'mature' sciences. Although Positive economists admit that there is a difference in the nature of the phenomena between the natural and social sciences, they claim that the difference is not of a kind but of a degree. According to Samuelson "There are no separate

methodological problems that face the social scientist different in kind from those that face any other scientist"<sup>23</sup>. By this claim Positive economists mean that economic propositions, whether these are the conclusions of deductive systems or inductive generalizations about regularities in the economic world, do not differ from physical propositions, due to the important property that, unlike normative propositions, they can be expressed in a positive manner so that they can say something about observable reality and thus can be tested with empirical evidence<sup>24</sup>. In other words, according to this claim, a sort of experiment may be set up in which theories or their predictions are tested through the comparison of the variables involved with the results of the experiment, while other (external) variables are held constant<sup>25</sup>. This method, therefore, demands the operationalization of theoretical terms which will provide economic propositions with the key to positive scientific testing<sup>26</sup>.

Needless to say that Positive economists realise the difficulties facing the experimenter in the social sciences and the consequent problems of unpredictability and uncertainty due to the human factor<sup>27</sup>. But they claim that this is not an obstacle to experimentation as evolving techniques appropriate to human experience will fulfil the necessary requirements for quantification and experiment<sup>28</sup>. Even though the conclusiveness and exactness of laboratory experiments is lacking, it is believed that the results from the application of methods devised for human experience, will serve as approximations for establishing valid economic theories. In this way economic theory can claim to be as objective as the natural sciences<sup>29</sup>.

The vision of scientific procedure that emerges from the above description of Positive economics is one which sees science developing "in an endless relation of give and take . . . (between) factual work and 'theoretical' work, . . . naturally testing one another (and which) will eventually produce scientific models"<sup>30</sup>. In this vision although theory is important, the factor that is assumed to be unquestionably the most decisive one, and the one that determines scientific advance, is the one that "relates questions to evidence"<sup>31</sup>.

b. THE MODEL OF SCIENTIFIC PROCEDURE IN POSITIVE ECONOMICS

This particular "give and take" between the "factual and the theoretical", the inductive and the deductive, has been outlined in an illuminating article by Machlup<sup>32</sup>. While talking about verifiability in economics he structures the entire theoretical and empirical apparatus of economics into two main parts<sup>33</sup>: the first, called Assumed and Deduced change, is situated at the beginning and the end of the structure. It has two separate functions: to provide observational data of a change and its consequences, as input to the second part, and to furnish conclusions derived from the second part, in terms of probable empirical "deduced effects". The second part includes the Assumed conditions which describe the way in which the Assumed change operates in terms of technological and organizational conditions. It further includes the Fundamental Postulates of the apparatus, which are supposed to be fixed. These fixed assumptions are the so-called ideal-types, such as, for example, "rational man", "perfect competition", etc. Their character is

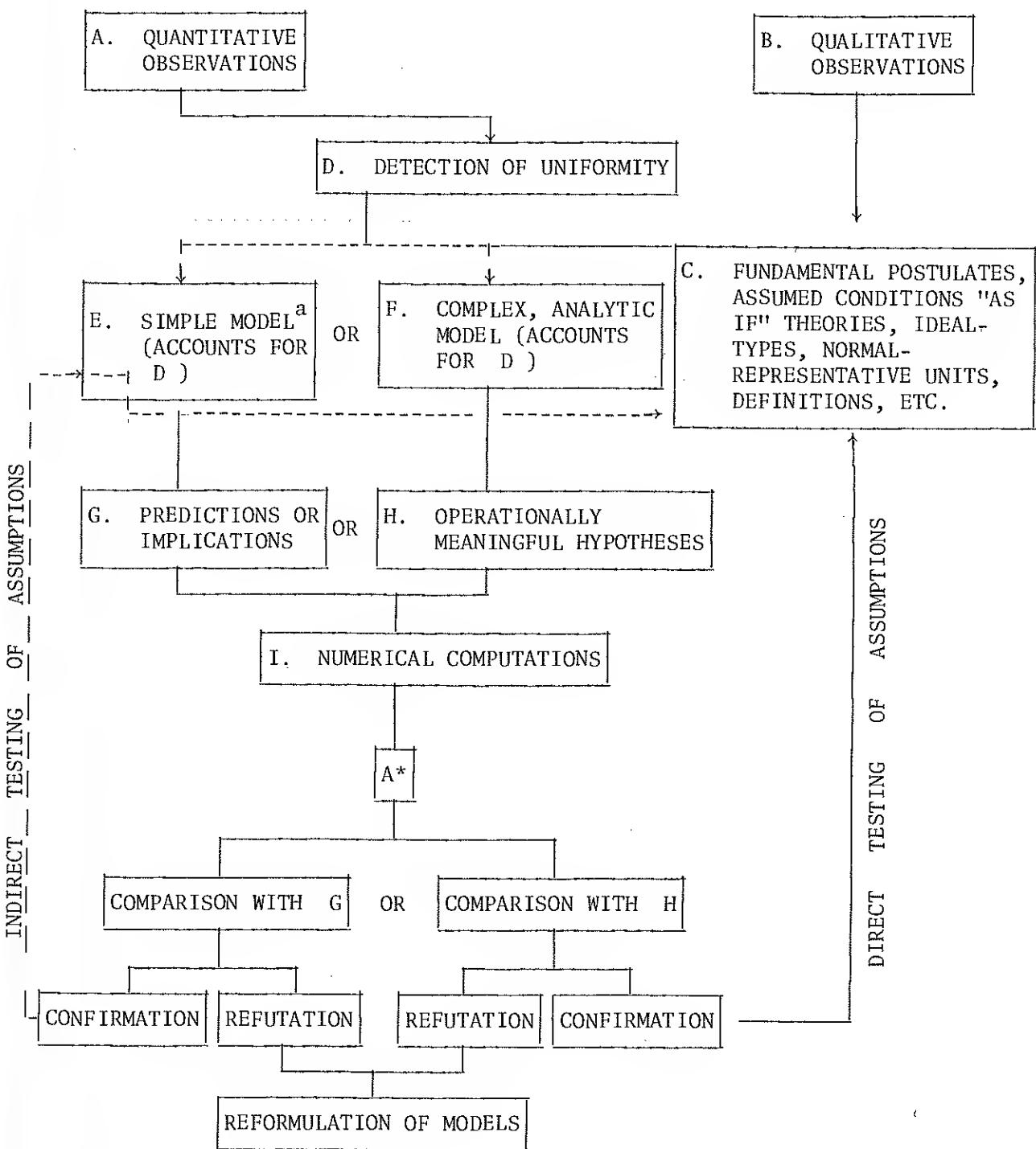
mostly a priori and their function mainly heuristic. Ideal-types, as Machlup explains, may be both a priori and empirical. A priori in terms of their abstract properties and their heuristic, analytic, value, and empirical in terms of being representative of a real type of action, assuming, in a Marshallian way, most of its 'relevant' characteristics and abstracting from the rest (this point about the dual nature of Fundamental Postulates is important because it sheds light on the F-Twist controversy, to be discussed in the next section). Moreover their validity is tentative, and they hold as long as their explanatory and predictive function, in terms of the totality of events they purport to explain and predict, is not replaced by more general postulates that could conceivably explain more phenomena with less assumptions, or predict better. This criterion, along with the one that their validity should be tested indirectly (through their predictions) against empirical evidence, according to Machlup, form the testing criteria of Fundamental Postulates<sup>34</sup>. The first and the last part of the apparatus, described by Machlup, are the ones that should be independently and directly tested against empirical data, since their terms are composed of inferences directly drawn from experience. The second part, i.e. the Assumed conditions and the Fundamental Postulates, should be checked only indirectly through their predictive capacity. Thus irrespective of whether these assumptions are 'real' or not, their validity holds as long as they generate correct predictions.

Although for Machlup, and also for Friedman, the validity of these assumptions is tested indirectly, it is not at all clear how they

are selected in the first place. Friedman, for example, would claim that their heuristic value cannot be determined ex ante, but only ex post through their predictive capacity. However, how can we have correct predictions, proving the heuristic value of the postulates, unless we have some initial assumption from which to deduce them? Before predictions are derived there must be an ex ante choice between alternative sets of Fundamental Postulates<sup>35</sup>. For instance Grah1 argues that "the choice of a model is necessarily prior to the assessment of its correspondence to the data"<sup>36</sup>. I think that this is a moot point in the methodology of Positive economics which, paradoxically, illuminates the deadlock in the F-Twist controversy. However, before I discuss the implications of this argument for the conflict between Assumptionists and Predictionists in greater detail, I shall try to elaborate on the model of scientific procedure in Positive economics in order to set the perspective for the discussion of the F-Twist controversy.

In Figure I, on p.26, I have attempted to construct the process of theory selection in Positive economics in such a way so as to accommodate both Friedmanian and Samuelsonian positivism. In contrast to Machlup's model which emphasizes only indirect testing of assumptions, my depiction of the procedure allows for both direct and indirect testing. The point of this exercise is to indicate that, despite differences concerning assumptions, the two versions of Positive economics have a common denominator which is empirical, quantified, facts.

FIGURE I: THEORY SELECTION IN POSITIVE ECONOMICS: AS SEEN FROM THE POINT OF VIEW OF BOTH ASSUMPTIONISTS AND PREDICTIONISTS



NOTE: a. I use the generic term model for both cases, because levels E and F are meant to convey the ideas of theory or hypothetical explanation, (as used by either Predictionists or Assumptionists), which usually are component parts of the structure of a model. Simple models are usually associated with Predictionists, who prefer a single-equation depiction of the economy and great predictive capacity, while complex models are associated with Assumptionists, who prefer large-scale models describing the economy analytically and realistically.

The top part of the diagram, A and B, constitutes the source of information for the observer<sup>37</sup>. A and B together form the building blocks for the construction of economic hypotheses<sup>38</sup>. The first source of information, A, consists of numerical data found in official statistics, results from previous hypotheses<sup>39</sup>, business reports, responses to questionnaires and in general the results of empirical investigations. This source of information, along with more intuitive and qualitative observations, forms the foundation of economic hypotheses. These hypotheses function as rationalizations of uniformities in the observations of economic events, hence level D in the diagram. The nature of the second source of information B, is largely a priori (self-evident) and is the source that feeds the observer with the theoretical assumptions and conditions that are necessary for sifting out the complex empirical observations in A. This source of information, through level C, helps the observer design the appropriate models, in E or F, that will account for the uniformity. Source of information B feeds level C with assumptions that consist largely of ad hoc impressions<sup>40</sup>, self-evident experience<sup>41</sup>, or normal-representative economic units<sup>42</sup>. Koopmans for instance claims that "the 'facts' of economic life are all around us. Hence much of the factual background of economic life can be presupposed without extensive examination or discussion"<sup>43</sup>.

Positive economists, depending on whether they are Predictionists or Assumptionists, will either use these assumptions as 'as if' and even 'unrealistic' constructs - the validity of which will depend upon the conformity of their predictions with experience -

or use them as ideal-type theories that represent real conditions approximately. Their validity will depend on their 'meaningfulness' in producing the ideal conditions under which a hypothesis could be tested<sup>44</sup>.

After observations, stemming from A, are ranked and ordered through the assumptions in C, either a simple equation or a complex model is constructed, which accounts for the possible regularities encountered in the observed events. The next step is to test these models against empirical evidence. Predictionists, following Friedman and Machlup, draw the implications or predictions and see whether the model is successful in predicting accurately. If it is, then it is confirmed until a new model based on greater simplicity, generality and predictive fruitfulness replaces it. This also constitutes indirect testing of the 'as if' assumptions in C. However, the success of the predictions of the model does not establish the truth value of the assumptions in C, but only confirms their heuristic value. On the other hand, Assumptionists, following Samuelson, construct a complex model disaggregating most sectors in the economy so as to approximate real conditions as closely as possible, and provide an analytical picture of the economy. This model contains operationally meaningful hypotheses<sup>45</sup>, which are tested against empirical evidence. The confirmation of these hypotheses establishes the validity of the model and serves as a direct criterion for establishing the approximate reality of ideal-type or deductive assumptions, such as the maximization-of-returns assumption.

The common denominator of the two approaches, and the unquestionable criterion against which predictions or hypotheses are to be tested, is empirical reality A\*. This is data independent of A (i.e. collected independently of A) accumulated either from compiled statistics or empirical investigations, in order to provide fresh empirical ground for the testing of the proposed models.

The complexity of Figure I is justified by the complexity of the Positive economic edifice constructed so as to safeguard objectivity, secure scientific procedures and discard ideological ramifications. However, in my view, there are two points in the diagram which disrupt the scientificity of the procedure. Firstly, level C in the figure provides a framework or a 'world-view' through which to perceive the unordered observations. It is by analogy the spectacles of the economist through which he/she views the world. The assumptions contained in this framework form the foundations upon which the models are constructed<sup>46</sup>. For example, in the case of macroeconomic models, consumption and investment functions are structured according to the fundamental assumption of diminishing returns. Or in the case of simpler models, prices are related to the money supply forming an hypothesis based upon the quantity theory of money<sup>47</sup> (albeit in the new empirical form given to it by Friedman), in which if velocity is assumed constant then neoclassical conditions such as perfect competitions and full employment are assumed.

Thus, a framework or an outlook is presupposed from which models are generated. The moot point about the choice of the framework

is that Positive economists do not provide convincing ex ante criteria for its selection among alternative ones, but only ex post empirical criteria. For example they claim that since the framework chosen works, then it is not necessary to look for another one. However, if in the first place 'works' means predicts accurately, then there is no assurance that there may not exist another framework that 'works', i.e. predicts, better. In the second place, if it means 'works for the purpose in hand', it is not at all clear why it works better for the purpose in hand, since no other framework has been tried other than the neoclassical framework. In other words, before the framework is tested with empirical criteria, one needs to justify the particular framework chosen among an infinity of alternative frameworks. For instance a Marxist framework emphasizing dynamic (historical-dialectic) disequilibrium states in society, and providing different ideal-types about economic relations, would generate a different set of models or predictions, that would need a different kind of evidence to be tested. And this brings us to the second weak point in the diagram: level A\* .

Does empirical data in A\* represent 'reality' as a whole? Marxists, for instance, would argue that this is not the case because of the two-level reality assumed by Marxist epistemology. The one pertains to a 'surface' and the other to an 'essential' reality. Empirical evidence of the positivist kind, represents surface reality, and if it is taken to represent the whole of reality then it obscures and hides the essential one. For Marxism, as we shall see in the following chapter, evidence comes from the unravelling of the essential

part of reality and the idea of 'praxis', which contains a dialectic between theory and socio-historical practice. Consequently, positivist observations of empirical reality are not and can not be considered objective criteria for Marxist theories. Theory selection in Marxism provides different criteria of objectivity. But even within positivist practice the quality, accuracy and the testing validity of empirical evidence is disputed. However, these points are related to the nature of empirical evidence, and their discussion will be postponed until chapter 2.

The important characteristic to be remembered in reference to scientific procedure in Positive economics is that, although theories have an important role to play within it, it is empirical, quantified and observable evidence that has the predominant function of being the absolute criterion of objectivity. And this is so irrespective of the position of the Positive economist in the F-Twist debate.

B. CONFLICT BETWEEN THE A PRIORI AND THE A POSTERIORI  
a. THE F-TWIST CONTROVERSY

Having acquired a general view of how Positive economics works in selecting between alternative theories, we can now proceed to discuss the methodological importance of the F-Twist controversy, and with the help of the above analysis, try to situate the position of the Assumptionists and the Predictionists within the Positive economic framework. My purpose is to show that the irresolution of

the controversy hinges upon a structured ambiguity situated in the positivist definition of 'Fundamental Postulates' or 'assumptions'.

An indication of the importance of this methodological conflict in economics is its long and persisting continuation.

Going as far back as the late 19th century we can see that methodological controversy was raging between the English and Austrian

Marginalists and the German Historical school, the so-called

'Methodenstreit'<sup>48</sup>. Then, as now, the issues were about the role of theory and abstraction in economics. In fact the 'Methodenstreit'

never ceased and was carried on in America between Institutionalists and Positivists. The clash between theoretical generality and

empirical relevance underlined the 'empty box' controversy in the

'30s, mainly in the Economic Journal<sup>49</sup>. In this controversy, behind

the discussion of neoclassical issues such as the representativity

of the firm, the returns to scale and perfect and imperfect competi-

tion, lay the methodological issues of the degree of theoretical

abstraction and empirical relevance. Although by the '40s some of

the issues in the above controversy seemed to have been settled and

imperfect competition became part of the economic orthodoxy, remnants

of the debate still lingered in the pages of the American Economic

Review, with the protagonists being Machlup and Lester discussing the

relevance of Marginalism<sup>50</sup>. While this debate was largely economic

in character, the explicit methodological content of the discussion

increased. The issues of theoretical versus purely empirical eco-

nomics became, yet again, prominent with the appearance of Friedman's

article on "The Methodology of Positive Economics". This rekindled

the old controversy and brought to the foreground issues concerning the epistemology of Positive economics<sup>51</sup>. From then onwards methodological conflict became an everyday feature of every major economic journal all over the world. Especially the conflict took a definite methodological form concerning theory and realism, what was later dubbed by Samuelson as the F-Twist controversy<sup>52</sup>.

The major controversial issue in this debate concerns the extent to which we need to test economic assumptions directly.

Friedman says that we do not; the testing of the predictions generated from these assumptions is sufficient to guarantee the heuristic validity of the assumptions. Assumptions, according to Friedman, need not have a one-to-one correspondence with empirical reality, but they should be couched in terms of 'as if' principles, simplifying reality and most of the time unconnected to it. In his words,

the relevant question to ask about the "assumptions" of a theory is not whether they are descriptively "realistic", for they never are, but whether they are sufficiently good approximations for the purpose in hand. And this question can be answered only by seeing whether the theory works, which means whether it yields sufficiently accurate predictions . . . To be important . . . a hypothesis must be descriptively false in its assumptions; . . . in general, the more significant the theory, the more unrealistic the assumptions . . .<sup>53</sup> (Hence the name 'F-Twist').

On the other hand, Samuelson declares that 'false' assumptions, in the above sense, cannot generate valid predictions. Accordingly, assumptions have to be checked for their empirical relevance before predictions are drawn, i.e. assuming simplicity, consistency and generality, assumptions should also contain terms that correspond to

reality: "the validity of the full consequences of a theory implies the validity of the theory and so of its minimal assumptions"<sup>54</sup>. In other words, a theory containing false assumptions cannot generate consequences that are either logically or empirically valid.

The issue, i.e. should assumptions be tested or not, seems to be clear. Friedman wants to test predictions whereas Samuelson wants to test assumptions. But is it? In order to answer this question I have first to examine what is the meaning of assumptions, predictions, hypotheses and theories, within the context of the distinctions made between statements such as, for example, logical-conditional statements, empirical-correlational hypotheses, primitive and secondary propositions and Fundamental Postulates<sup>55</sup>. Also I shall briefly review the criteria of the truth conditions for these statements<sup>56</sup>.

Firstly, logical-conditional statements are comprised of an antecedent clause p and a consequence q implying a logical relationship. The proposition 'if prices are given to the firm and there is perfect competition, then the firm will choose, ceteris paribus, the output that maximizes its profit' is an example of the proposition of the form 'if p then q , assuming p and ceteris paribus conditions'. For the consequence to follow from the premise there are certain logical-deductive laws that define the rules with which we establish the logical truth of a logical-conditional statement<sup>57</sup>. That is, if the premise p is true then necessarily q is

true, and if  $p$  is false then also  $q$  is false. In a sense the consequence of such statements is already implied in the antecedent and is drawn out by logical rules.

On the other hand, hypotheses involving terms describing events in the empirical world are called empirical-correlational hypotheses. Although their form resembles the form of logical-conditional statements, it differs in that the  $p$ s and  $q$ s are empirically interpreted. This can make a great difference as it implies that the truth conditions for this type of hypotheses follow different rules. The terms in these hypotheses represent, in a symbolic manner, occurrences of events in the 'real', i.e. empirical, world. The relationship between any two events of this kind is considered associational. This is so because inference for these hypotheses follows inductive rules<sup>58</sup>. Inductive rules obey laws that are characteristic of the empirical realm. Since empirical reality is assumed as consisting of contingent events causally, i.e. mechanistically, unrelated, the representational form of these events should be associational. In other words, since in the empirical world there is neither inductive nor deductive assurance that even  $q$  will occur when  $p$  occurs, the only assurance one can depend on is probabilistic; and this implies that the relationship between events  $p$  and  $q$  is one of a degree of association. An economic example of this kind of hypotheses is the restatement of the Quantity Theory of Money by Friedman in terms of an empirical function relating the demand for money with certain empirically defined variables<sup>59</sup>. From this an empirical hypothesis (or prediction) follows correlating a

measure of the supply of money with the level of prices. The terms in this hypothesis are empirically defined and their link is probabilistic. The truth condition for these sentences is simply empirical evidence. However, as Clarkson points out, while pleading for an empirically testable consumption function, all terms within an empirical hypothesis (or prediction) have to be empirically defined and confirmed<sup>60</sup>. In other words, if one wants to find out that the rise in money systematically precedes the rise in prices, then one first has to find out that indeed the money supply and the price level rise. In cases where this is not true, i.e. one of the terms is not confirmed, one cannot have antecedent clauses that are heuristically (i.e. have an 'as if' value) or descriptively false, for the truth of the statement depends on all its terms being directly confirmed by experience.

Another distinction relates to protocol or basic sentences<sup>61</sup>. These sentences contain terms that are directly linked with the world, i.e. the terms represent experience in a one-to-one correspondence. A basic sentence is different from an empirical-correlational hypothesis in that it does not require any previous theory to act as framework or background<sup>62</sup>. Whereas an empirical-correlational hypothesis is formulated with concepts and implications borrowed from an antecedent theory, e.g. the money-prices association bases itself on the Quantity Theory of Money and ultimately on the neoclassical theory of value, a primitive sentence is 'purely' experiential and independent of any theory<sup>63</sup>. For example the statement that, while demand is kept unchanged, the price of meat fluctuates with changes in trade union policies, is assumed to relate to an observation that is supposed to

be generated from experience and not from theory. The scope of this sort of statement is limited to the observations contained in its terms and its extent covers the particular phenomenon in question, having no general implications. The truth conditions for these statements are the same as for the empirical-correlational hypotheses.

In contrast, secondary or theoretical sentences, as Massey asserts, "though tied semantically to experience, do not enjoy the same direct relation thereto that characterized the basic sentences"<sup>64</sup>. The terms contained in these sentences though referring to actual events, are abstract entities and cover a wide and general domain. The Keynesian theory of employment or the neoclassical theory of consumer choice are cases in point. In both theories although the terms involved describe an event occurring in the actual world, they are constructed from a general and abstract point of view, approximating and simplifying reality and bypassing all kinds of minor observations. One cannot expect, for example, that the specified consumption or investment functions in the Keynesian theory should be rejected because a particular entrepreneur does not borrow when interest rates go down, or that a consumer does not act as specified in an indifference map. As Nagel affirms "a theoretical statement having this form is not proved to be false by showing that the specifications in the antecedent are not embodied in some given spatiotemporal region"<sup>65</sup>. In other words, the antecedent clause of such statements amasses the dominant and general characteristics of a phenomenon and abstracts from the less important ones. However, this does not mean that the

assumption of a theory is in any way unrealistically false, i.e. that there is no element at all in the real world denoting a maximizer of economic returns. Theories are based upon general and simplified observations related indirectly to reality and ordering its complex manifestations. The truth conditions of such statements, as well as of Fundamental Postulates, are empirical evidence directly testing their predictions and indirectly their antecedent assumptions. (This is one issue that divides Positive economists into an Assumptionist group who want to test assumptions directly, and Predictionists who want to test assumptions indirectly.) The important point to note about these statements is that these assumptions approximate reality and are true, i.e. have a bearing on reality. According to Nagel "statements containing such terms [i.e. theoretical] cannot possibly explain or predict the course of actual events, unless a sufficient number of theoretical terms (but not necessarily all of them) are co-ordinated with observable traits of things"<sup>66</sup>

However, in my view the difficulty with abstraction and simplification in these statements, as in the case of Fundamental Postulates, is that one does not know, at least a priori, the criteria by which one is supposed to select the most important and leave out the less important aspects of a phenomenon. Is the political awareness of the entrepreneur, or the knowledge about the nature of a commodity, or advertising, important characteristics or not? Some people argue that they are<sup>67</sup> and some that they are not<sup>68</sup>. It is difficult to select between the two approaches because under different criteria we have different definitions of importance. Moreover we

cannot take the pragmatist position and argue that the most important characteristics are the ones that work, because we need to select some characteristics from an infinite array before we put them into the pragmatic test. Besides, theories of the firm that are labelled 'organizational' seem to be working as well as the neoclassical ones<sup>69</sup>.

The most important category of statements, as far as the F-Twist controversy is concerned, relates to what has been commonly called Fundamental Postulates. A collection of such postulates has the significant property of structuring the context or the perspective from which theories and hypotheses are generated, hence, what Melitz calls "generative assumptions"<sup>70</sup>. These assumptions are what Weber called "ideal types"<sup>71</sup>, or Robbins the "generalizations" of economic theory<sup>72</sup>. Irrespective of whether one is a purely empirical or a purely theoretical investigator, one always shapes one's empirical hypotheses, or one draws one's theories, from the pool of such statements. For example, in the case of Neoclassical economics there are certain assumptions such as 'rationality' and 'scarcity' that form the pillars supporting the whole theoretical system. If 'irrationality' of economic behaviour and 'abundance' is assumed, then the forcefulness of the theories of the firm and of demand is greatly diminished.

The problem with these assumptions is their truth conditions. Are they logically true (i.e. tautologies, empty of empirical meaning), and therefore abide by the rules stipulating the truth conditions of logical-conditional statements, or are they assumptions partly submerged in abstraction and partly in experience, in which case the validational

procedure is different? As we shall see in the course of the section, there is ambiguity as to the meaning and use of the term 'assumptions'.

If we take for instance the proposition "man is economically rational because of scarcity" are we assuming that the proposition is (a) only an ideal construct approximating reality, or (b) "descriptively false" and used for heuristic purposes, or (c) a Marshallian "representative" or "average" tendency, or lastly, (d) as Robbins claims, a "self-evident" universal proposition<sup>73</sup>? Whatever our answer be to the above question, in each case there is an ambiguity which has been an important factor in causing the persistence of the F-Twist controversy<sup>74</sup>.

If we take each one of the four cases we shall see that in all of them always there are two elements involved: one purely logical and one purely empirical. The purely logical concerns abstraction and analytical function and the purely empirical concerns an ontological (anthropological) statement about man in society. An ideal-construct, for instance, contains an element of logical abstraction and an element of empirical reality. 'Rational economic man', though extreme and general, refers to a particular situation where one can envisage a 'real' rational economic man. This duality is also manifested in 'descriptively false' propositions whereby although one omits 'the particular' details of an event, such as that some men may be economically irrational, nevertheless one refers to the 'real' essence of this event which may be remotely distant from manifest 'reality'. The same applies to Marshallian representativity. One represents in an average

and abstract sense the manifestations of a 'real' tendency. Similarly in the case of 'self-evident' truths, 'self-evident' refers to the obviousness and generality of the proposition, which if not true for a particular 'spatio-temporal region', still applies to most cases. This characteristic is common to all four cases and it can be subsumed under the following idea: in all cases it is assumed that, on the one hand although not all aspects of human activity are rational it can still be considered that economically (in a fundamental sense) man is rational, in order to derive, in conjunction with scarcity, marginalistic hypotheses about changes and their effects (the logical part), and on the other hand it is assumed that although there are variations and degrees of rationality, man is ultimately a selective animal, one that makes constrained choices (the ontological 'real' part).

However, if the structure of such assumptions is comprised of two antithetical elements, assumed versus real rationality, ideal versus anthropological rationality, then to which one should truth conditions be addressed? Do we need to empirically justify the realism of the assumption that man is indeed ontologically rational? And if we do not, what justifies the selection of this particular postulate as opposed to any other one? For example, why can we not replace the 'rationality' principle with the principle of a Marxian 'mode of production', or any other principle incorporating 'irrationality', e.g. psychoanalysis, as a Fundamental Postulate? To say also that the selection of the principle is justified a posteriori, i.e. through its predictions or explanatory power or analytic capacity, is to simply beg the question, because one is not given the criteria according to which

the principle was selected among an infinity of potentially predictive, explanatory and analytic principles. Also if the objection is that the economist is not omniscient or that he/she cannot use all possible alternatives at once, then why did he/she select the particular principle of economic rationality, and why did he/she (presumably orthodox economist) not use any other alternative principle since the emergence of Neoclassical economics?

On the other hand, if the selection is done according to purely logical principles, then the statement (according to positivist principles) is tautologous, i.e. logically necessarily true, deductively conceived, and it does not matter whether it is empirically unrealistic. Man may not in reality be as portrayed in Neoclassical economics, but the conception that emerges from the postulate of 'rationality' allows the logical manipulation of theories, following deductive rules and analytical procedures, and permitting their test only indirectly through their predictions. As Friedman contends their value is heuristic and their format "as if".

But is this the case? Are fundamental postulates purely logical? If this is the case, then their terms can be exchanged by other terms, with the same logical value, implying the same logical relationship. For example, the proposition "if  $A=B=C$  then  $A=C$ " may be easily converted to the proposition "if  $D=E=F$  then  $D=F$ ", without making any logical difference. If this is the case, then why is 'rationality' a term always in the limelight while others are not? Moreover, could the neoclassical set of terms be exchanged with any other set of terms without making any logical difference? In other

words could the term 'rationality' be exchanged without altering the fundamental structure of the neoclassical system? I think not, because Neoclassical economics focuses on 'rationality' as a real situation, and justifies it with an appeal to ontological reality. But if this is so then we have to turn to Fundamental Postulates containing both logical and ontological elements, in which case the truth conditions are again ambiguous. That is, should such an assumption be empirically realistic or theoretically unrealistic? If both, then one needs criteria to decide between the realism and the unrealism of the assumption. In other words, one needs criteria in order to demarcate how representative or approximate or descriptively false of reality is the assumption. Are all men, some men, or none, rational?

In my opinion this ambiguity provokes a dichotomy that splits the economists between opposing camps supporting either the realism or the unrealism of Fundamental Postulates. In fact, as has been noted by Nagel and Massey, and as we shall see next, even within each camp economists are ambiguous concerning the meaning of Fundamental Postulates.

Having, therefore, distinguished between various kinds of statements in Positive economics, and having indicated a clue towards the understanding of the methodological conflict within the paradigm, I can now proceed to examine how Positive economists define and use the meaning of Fundamental Postulates, in connection with this conflict.

## b. THE REALISM OF THEORY AND THE THEORY OF UNREALISM

because we do not know the truth . . .  
 we make falsehood as much like truth as  
 we can.

Plato, Republic<sup>75</sup>

The fundamental question in this section is whether Friedman and Samuelson are consistent in their use of the terms 'assumption' and 'theory'. If they are not, my hypothesis will be that the inconsistency derives from the above-mentioned ambiguity in the structure of the term 'assumption'.

Does Friedman really mean that assumptions should be unrealistic? And that the more unrealistic assumptions are, the more fruitful they are in terms of the predictions they generate? It is obvious from his remark that "the more significant the theory the more unrealistic the assumption"<sup>76</sup> that, at the limit, the more we abstract from empirical reality and we construct theories that diverge from it, the more we can generate correct predictions about empirical reality. At the limit, completely false theories, e.g. that the earth is flat or that individuals act independently of each other's economic decisions<sup>77</sup>, would generate the greatest number of testable predictions<sup>78</sup>.

However, Friedman qualifies this seemingly paradoxical consequence, i.e. false assumptions-correct predictions, by saying that "the relevant question to ask about the 'assumptions' of a theory is not whether they are descriptively 'realistic', for they never are, but whether they are sufficiently good approximations . . ."<sup>79</sup>. But

what does Friedman mean by "assumptions"? From the context of what he says he means Fundamental Postulates, in the sense described in the previous section. For if he meant the antecedent clauses of basic sentences or empirical-correlational hypotheses or theories (he could not have meant logical-conditional statements because of the objections raised in p.42), he would have had to concede to the objection raised by Samuelson, Nagel and Melitz to the effect that (empirically or logically) true consequences stem from true premises. Consequently if Friedman means Fundamental Postulates by "assumptions"<sup>80</sup>, then there is sufficient leeway within the structure of these postulates to allow him the implication of the 'unrealism' of assumptions.

The key words in the above quotation by Friedman are "descriptively false", which means that for the purposes of theoretical understanding and generality, reality is reconstructed ideally with terms used either to approximate empirical description or reorder it so as to give meaning to empirical phenomena. The maximization principle (implying rationality) is a case in point whereby although one does not encounter any real economic agent actually maximizing profits or utility, reality is somehow represented approximately. This principle, along with the rest of the abstract neoclassical principles, provides the framework which, according to Friedman, can be used to generate testable predictions.

However, the question remains whether the principle is truly false or simply an approximation of true reality, and an ideal construct implying a divergence from reality at the limit? If we follow

Friedman's theory of 'unrealism', the epistemological abstraction (ideal-construct) is that: 'false' assumptions (in an ideal sense) generate potentially true predictions. For Friedman a good 'false' assumption is differentiated from a bad one if it "works, which means whether it yields sufficiently accurate predictions"<sup>81</sup>. However, as I have already pointed out, we cannot know before the predictions are generated, which 'false' assumptions to select.

Friedman answers by referring to the other side of Fundamental Postulates, namely the one relating to the approximate and ideal-type truth of assumptions. An assumption, according to this, may be only descriptively false, meaning that its reality is embedded in a remote empirical corner, abstracting from the inessential and selecting the 'true' essential. Thus it seems that Friedman, although he takes for granted that the assumptions will involve hypotheses such as perfect competition or that every economic agent is a maximizer, nevertheless thinks that these assumptions should also be tied to actual rather than fictitious conditions. For example, independent utility functions or perfect competition or economic maximizers might not exist but it can be assumed as if they exist by way of approximation, for the purposes in hand. 'By way of approximation' means that there is some real entity which one simplifies by idealizing it.

Thus, apart from the indirect truth conditions stemming from the testing of predictions, assumptions have a sort of direct truth conditions, confirming the choice of the 'real entity', which rely on "evidence of a very different kind"<sup>82</sup>. For example evidence such as

"intuitive plausibility"<sup>83</sup>, or that "a judgement may be required before any satisfactory test . . . has been made, and, perhaps, when it cannot be made in the near future, in which case, the judgement will have to be based on the inadequate evidence available"<sup>84</sup> (my emphases), or evidence of the nature that "unless the behaviour of businessmen in some way or another approximated behaviour consistent with the maximization of returns, it seems unlikely that they would remain in business for long"<sup>85</sup>. These are some of the evidence or criteria that Friedman uses for the a priori selection of assumptions.

However, there is a sort of ambiguity and inconsistency in Friedman's epistemological dicta concerning assumptions in economics. The ambiguity refers to what I have called the 'strong' and the 'weak' axioms of the theory of unrealism'. The 'strong axiom' refers to the choice of assumptions on the basis of prediction tests, and the 'weak axiom' to the choice of assumptions on the basis of largely 'intuitive', 'common-sense', 'self-evident' knowledge. While Friedman feels secure when he talks about the 'strong axiom', because he knows that what matters is predictive capacity irrespective of the realism of assumptions, he feels less so when he realizes that assumptions must somehow be selected and justified a priori, in which case he needs the 'weak axiom'. The reason he has to do so is that he has to believe in some sort of ontological truth for his assumptions because they concern human action. While with inanimate objects the "as if" formula acquires a purely explanatory and analytical function (like Friedman's leaves behaving "as if" they are maximizing their position in relation to the sun) with no real purposive action attributed to the object,

with human action the "as if" formula has both a fictitious, due to the ideal form given by the theorist, and a real dimension, due to the actual purposive action of human behaviour<sup>86</sup>. Clearly the ambiguity referred to as the 'strong and weak axioms of the theory of unrealism', is due to the looseness of the definition of the term 'assumption', which does not contain a precise criterion as to the realism or unrealism permitted in constructing economic postulates.

On the other side of the controversy Samuelson reproaches Friedman for his theory of unrealism, and insists on the reversal of the two axioms. Namely that the strong axiom should be the realism of assumptions and the weak axiom should be the testing of predictions. As Samuelson claims "the whole force of my attack on the F-Twist . . . is that the doughnut of empirical correctness in a theory constitutes its worth, while its hole of untruth constitutes its weakness"<sup>87</sup>. But before I examine Samuelson's position vis a vis the ambiguity involved in the definition of Fundamental Postulates, I shall firstly examine what he means when he employs the term 'assumption'.

Paradoxically Samuelson uses the term 'assumption' in the same way Friedman does<sup>88</sup>. Why is he then insisting on testing the premise before testing the conclusion? If he was claiming for this only, then by 'assumption' he would have meant logical-conditional statements whereby conclusions are already implied in the premises, or primitive-basic sentences whereby both assumptions and conclusions require empirical confirmation. However, Samuelson does not mean these kinds of statements when he says that "Every theory . . .

distorts reality in that it over-simplifies"<sup>89</sup>, therefore implying that 'assumptions' may 'idealise'<sup>90</sup> or 'abstract'<sup>91</sup> or 'approximate'<sup>92</sup>, in which case by 'assumptions' he really means Fundamental Postulates. More exactly he means those conditions that function as parameters in a given system, and act as the Fundamental Postulates of the system. That is "values [of unknown variables] emerge as a solution of a specified set of relationships imposed upon the unknowns by assumption or hypothesis . . . we assume implicitly a matrix of conditions within which our analysis is to take place"<sup>93</sup>. This matrix of conditions, according to Samuelson, boils down to the principle of maximization. Meaningful, i.e. refutable, theorems are deduced largely from this principle<sup>94</sup>. "Many of these stability conditions rest implicitly upon maximizing behaviour"<sup>95</sup>. Having said that he goes on to argue that this principle can be used as an assumption "even though it is admittedly not a case of any individual's behaving in a maximizing manner"<sup>96</sup>, because, as he puts it in a different context, "to say 'Galileo's ball rolls down the inclined plane as if to minimize the integral of action or to minimize Hamilton's integral', does prove to be useful to the observing physicists, eager to formulate predictable uniformities of nature"<sup>97</sup>.

But then, if assumptions, or conditions, can be formulated in an 'as if' manner, or act as ideal constructs, or approximating realities, exactly as Friedman prescribes, where is the controversy? How does Samuelson differ from Friedman? Why should Samuelson answer by claiming that he is "objecting to the requirement that a usefully realistic theory must be completely accurate? Who seriously thinks

otherwise?"<sup>98</sup> Or that his "own position never rejected approximating concepts"<sup>99</sup>, but that "the validity of the full consequences of a theory implies the validity of the theory and so of its minimal assumptions"<sup>100</sup>. Thus, although Samuelson accepts 'descriptively false' assumptions diverging from empirical reality, he differs from Friedman in that he admits only plausible rather than false assumptions. However, it has been shown that Friedman also finally, but not decisively, accepted the intuitive plausibility of assumptions.

Is the controversy then only a verbal misunderstanding?

I think not. The controversy is about a substantial issue borne from the dichotomy implied in the definition of Fundamental Postulate. If, as indicated above, a Fundamental Postulate is a sort of assumption containing terms that are considered, in a sense, both true, i.e. plausible, approximate, evident; and false, i.e. ideal, as if, descriptively false, unrealistic, then the ambiguity and tension resulting from the imprecise specification (a specification which, however, as we shall see in chapter 7, is inherently imprecise due to an ambiguity in the whole of Positive economics) of the criteria under which such a postulate may be said to be realistic or unrealistic, allows the possibility of incompatible interpretations and therefore controversy<sup>101</sup>.

In my view although in both sides of the debate economic assumptions and their properties are defined more or less in a similar way, nonetheless the split between unrealistic abstraction and realistic plausibility, produces a tension that keeps the controversy unresolved.

Again at the limit, i.e. at the point where the opposing factions argue about the significance of false (and not only descriptively false) assumptions versus the significance of empirically true (and not only approximately true) assumptions, the tension becomes more clear-cut. For example, reviewers of the controversy refer to the opposing schools as "extreme a priorists" and "ultra-empiricists"<sup>102</sup>. However, these extreme positions, although closer to older methodological conflicts, do not apply in the case of the F-Twist controversy. In the latter a more balanced position is taken and the tension is significantly contained within the limits of having to find an appropriate (and most of the time unstable) equilibrium between theory and experience.

Having said that, it must be remembered that despite differences displayed in the controversy concerning, as Samuelson puts it, "the relationships between observable reality and the various assertions made by the scientific theory"<sup>103</sup>, the truth remains that the controversialists are united in their positivist epistemological belief that, although theory is important, it is always empirical facts that have the last word. In the words of Friedman "Only factual evidence can show whether (a theory) is 'right' or 'wrong'"<sup>104</sup>, and of Samuelson's "Let experience tell the final story"<sup>105</sup>.

To be able to see whether indeed "factual evidence" or "experience" constitutes the ultimate judge of Positive economic theories, we have to examine its status in practice in its role as a resolver of a major economic controversy. This, however, will be the

task of Part II. In the meanwhile we have first to examine the nature of empirical evidence and experience in general and as used in economics, and second to juxtapose a different epistemology (embracing a different and perhaps alternative set of Fundamental Postulates) which defines a different status and role for empirical facts.

### C. CONCLUSION

The continuing persistence of the F-Twist controversy has been a puzzle to many students of economic methodology<sup>106</sup>. Although different kinds of explanation may be given for the stalemate<sup>107</sup>, it seems to me that a plausible one relates to the ambiguity and resulting tension, that characterises the definition and structure of Fundamental Postulates, and their use in Positive economics.

However, as I shall attempt to show in Part III, this tension is not only situated in the Fundamental Postulates of Positive economics, but is part of a larger tension that springs out of a fundamental dichotomy running through the epistemological and economic assumptions of Positive economics as a whole. This dichotomy, producing a major tension, is necessitated by the positivist epistemological decision, in the face of the Inductive and Deductive problems, to make a complete breakdown, or thorough separation in the categories of theory and reality, between an abstract and a concrete, an imaginary and a real.

Consequently, the ambiguity in the definition of Fundamental Postulates cannot be resolved because it is a structural one, inherently built in, and essential for the viability of Positive economics.

## FOOTNOTES:

## PART 1:

## OUTLINE

1. See for example, L.E. Hill, "A Critique of Positive Economics", American Journal of Economics and Sociology, 1968, pp.259-66; A. Coddington, "Positive Economics", Canadian Journal of Economics, 1972, pp.1-15; E. Rotwein, "On the Methodology of Positive Economics", Quarterly Journal of Economics, 1959, pp.554-75. Hollis and Nell, "Rational Economic Man", op.cit. E.K. Hunt and J.G. Schwartz, A Critique of Economic Theory, 1972, R. Heilbroner, Is Economics Relevant? A Reader in Political Economics, 1971; B. Seligman, "The Impact of Positivism in Economic Thought", History of Political Economy, 1969, pp.256-278, et.al.

## CHAPTER 1:

2. J.N. Keynes, The Scope and Method of Political Economy, 1891, p.34, "we ought at least to recognise as fundamental a positive science of political economy which is concerned purely with what is" (my emphasis), ibid., p.36.
3. J.S. Mill, "On the Definition of Political Economy; and On the Method of Investigation Proper to It", in his Essays on Some Unsettled Questions of Political Economy, 1877, p.124, and Keynes, op.cit., p.35.
4. See for example P.A. Samuelson's Economics, 8th Edition, pp.8-9.
5. The problems confronting the observer of social phenomena will be discussed in chapters 2 and 6.
6. See, e.g. F.W. Knight, "What is Truth in Economics" in his On the History and Method of Economics, 1963, pp.158-160, and Coddington, "Positive Economics", op.cit., p.12.
7. R.L. Heilbroner, "On the Limited relevance of Economics", The Public Interest, 1970, p.93.
8. See, G. Myrdal, An International Economy, 1956, pp.337-340, also I.M.D. Little, A Critique of Welfare Economics, 1960, pp.79-80.
9. Hunt and Schwartz, "A Critique of Economic Theory", op.cit., p.14.
10. It should be mentioned, however, that although the enormous and complex edifice of Positive economics contains arguments that take account of such objections, it nevertheless, as we shall see, rests its foundations on assumptions that themselves may be biased, ambiguous and implausible.

## FOOTNOTES (cont.)

## PART I: CHAPTER 1:

11. J.A. Schumpeter, "The Common Sense of Econometrics", Econometrica, 1933, pp.5-7. Also according to Knight, "the concept or feeling of objectivity itself as against subjectivity, has come for the modern mind to be closely connected with the possibility of measurement", "On the History and Method . . .", op.cit., p.165. See also Machlup, "Are the Social Sciences Really Inferior?" in M. Natanson's Philosophy of the Social Sciences, 1963, pp.161-166, in fact in this article Machlup attempts to counter nine objections raised against the validity of Positive economics.
12. See for instance, J.A. Schumpeter, History of Economic Analysis, 1954, pp.31-43.
13. See, A. Marshall, Principles of Economics, 6th Edition, 1910, pp.31-37.
14. Friedman, "The Methodology . . ." op.cit., p.10, also C.F. Mills, "On Measurement in Economics", in R.G. Tugwell, The Trend of Economics, 1924, pp.38-9; see also G. Tintner, "Some Thoughts about the State of Econometrics", in Krupp, "The Structure . . ." op.cit., pp.121-123. Of course problems arise when one is confronted with inter-dependence of individual behaviour, as in the case of oligopoly.
15. P.A. Samuelson, "Economic Theory and Mathematics - An Appraisal", American Economic Review Papers and Proceedings, 1952, p.57, also Friedman, "The Methodology . . ." op.cit., p.7.
16. Samuelson, ibid., p.57, and his Foundations of Economic Analysis, 1965, p.4, also Friedman, ibid., pp.7-8.
17. Friedman, ibid., pp.4-5, also Lipsey, "An Introduction . . .", op.cit., pp.2-5.
18. Keynes, "The Scope and Method . . .", op.cit., p.13.
19. See Friedman, "The Methodology . . .", op.cit., pp.3-7. However, it escapes the Positive economist that when he/she draws the policy implications from a positive analysis of the structure of the economy, he/she in fact neglects the historical change of that structure, which means the neglect of alternative structures, alternative positive analysis and therefore, alternative policy implications. To accept the 'what is' is a value judgement in itself barring the positive economist from seeing any alternative 'what is'.
20. See P. Streeten's collection of Myrdal's essays on Value in Social Theory, 1958, especially the Introduction by Streeten. The criticism to the effect that Positive statements are value loaded will be scrutinized in the first section of chapter 6.
21. F. Machlup, "Positive and Normative Economics: An Analysis of the Ideas", in Heilbroner's "Economic Means and Social Ends", op.cit., p.111.

## FOOTNOTES (cont.)

## PART 1: CHAPTER 1:

22. Lipsey, "Introduction . . .", op.cit., p.5.
23. Samuelson, "Economic Theory and Mathematics . . .", op.cit., p.46, see also Friedman, "The Methodology . . .", op.cit., p.10.
24. Samuelson, "Foundations . . .", op.cit., p.4, and Lipsey, ibid., p.5.
25. Friedman, "The Methodology . . .", op.cit., p.10. Of course the question arises whether external variables can, in effect, be held constant. This is a question often asked by critics of Positive economics, see for example, K. Boulding, Economics as a Science, 1970, pp.1-22, E.H. Phelps-Brown, "The Underdevelopment of Economics", Economic Journal, 1972, p.5, Knight, "What is Truth . . ." op.cit., p.176, et al.
26. Samuelson, "Foundations . . .", pp.3-4, also F. Machlup, "Operationalization and Pure Theory in Economics", in Krupp "The Structure . . .", op.cit., pp.53-67.
27. "The much greater difficulty in securing adequate and convincing tests for statements and theories in the human and social sciences is, and seems will remain, a source of important differences. But these are differences of degree . . . and not of principle", T.W. Hutchison, The Significance and Basic Postulates of Economic Theory, 1965, p.XII.
28. Friedman, "The Methodology . . .", op.cit., pp.10-11, and Tintner, "Some Thoughts . . .", op.cit., p.38. For example some of these techniques are: simulation, stochastic methods, game theory, etc.
29. "[P]ositive economics is, or can be, an 'objective' science, in precisely the same sense as any of the physical sciences", Friedman, ibid., p.4. However, for the development of techniques Cohen, among others, has argued that it is "a persistent illusion that by some new trick of method the social sciences can readily be put on a par with the physical sciences . . . It is vain to expect that the crudeness of our observation the vagueness of our fundamental categories will be cured by manipulation of the paraphernalia of statistical methods", Reason and Nature, 1931, Book III, pp.350,353. See also A. Coddington, "Soft Numbers and Hard Facts", New Society, 1970, pp.579-581, and "Positive Economics", op.cit., pp.8-12.
30. Schumpeter, "History of Economic Analysis", op.cit., p.42.
31. Lipsey, "Introduction . . .", op.cit., p.5.
32. F. Machlup, "The Problem of Verification in Economics", The Southern Economic Journal, 1955, pp.1-21.
33. Ibid., p.13.

## FOOTNOTES (cont.)

## PART I: CHAPTER 1:

34. It must be noted, however, that these criteria have only a nominal value since theories and predictions have been tested in practice, without altering the fundamental assumptions behind them. The neoclassical postulates of scarcity and rationality have remained intact from their inception until today even if they have been shown to be false or irrelevant. (See E. Grunberg, "Notes on the Verifiability of Economic Laws", Philosophy of Science, 1957, p.342.) For a defense of the neoclassical assumptions against alternatives see K. Arrow, "Limited Knowledge and Economic Analysis", American Economic Review, 1974, pp.1-2.
35. For similar doubts see E. Rotwein, "Empiricism and Economic Method: Several Views Considered", Journal of Economic Issues, 1973, pp.371,380.
36. J. Grahm, "Econometrics and Economics", Unpublished draft, 1977.
37. Friedman, "The Methodology . . .", op.cit., p.4. The model is constructed according to the beliefs of Positive economists.
38. Ibid., p.12.
39. Ibid., p.14.
40. For instance, "Confidence in the maximization-of-returns hypothesis is justified by evidence of a very different character . . . This evidence is extremely hard to document; it is scattered in numerous memorandums, articles and monographs . . .", ibid., pp.22-23.
41. "[Assumptions] do not need controlled experiments to establish their validity: they are so much the stuff of our every day experience that they have only to be stated to be recognized as obvious." L. Robbins, An Essay on the Nature and Significance of Economic Science, 1949, p.79.
42. "The course of action which may be expected under certain conditions from the members of an industrial group is the normal action of the members of that group relatively to those conditions" or "a representative firm is in some sense an average firm", Marshall, "Principles of Economics", op.cit., pp.34,318. See also J.N. Wolfe's "The Representative Firm", The Economic Journal, 1954, pp.337-349. For critical comments about the "representativity" of economic units see Phelps-Brown, "The Underdevelopment of Economics", op.cit., p.2.
43. T.C. Koopmans, "The Construction of Economic Knowledge" in his Three Essays on the State of Economic Knowledge, 1957, p.131.
44. Samuelson, "Foundations . . .", op.cit., pp.4-5.
45. These hypotheses differ from purely theoretical hypotheses in that their terms are directly linked to the real world and thus can be empirically tested directly: "By meaningful theorem I mean simply a hypothesis about empirical data which could conceivably be refuted, if only under ideal conditions", Samuelson, "Foundations . . .", op.cit., p.4.

## FOOTNOTES (cont.)

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46. Ward, "What's Wrong . . ." op.cit., p.157, R. Meek, "Value in the History of Economic Thought", History of Political Economy, 1974, p.250.
47. See M. Friedman, A Theoretical Framework for Monetary Analysis, National Bureau of Economic Research, New York, 1971, Occasional Paper 112, p.1.
48. F. Machlup, "Theories of the Firm: Marginalist, Behavioral, Managerial", American Economic Review, 1967, p.3.
49. For a review of this controversy see R. Triffin, Monopolistic Competition and General Equilibrium Theory, 1947, pp.8-9.
50. F. Machlup, "Marginal Analysis and Empirical Research", American Economic Review, 1946, pp.519-524, "Rejoinder to an Antimarginalist", American Economic Review, 1947, pp.148-154 and R.A. Lester, "Shortcomings of Marginal Analysis for Wage-Employment Problems", American Economic Review, 1946, pp.63-82, "Marginalism and Labour Markets", American Economic Review, 1947, pp.135-148.
51. See for example Machlup's "The Problem of Verification in Economics", op.cit., and "Rejoinder to a Reluctant Ultra-Empiricist", Southern Economic Journal, 1956, pp.483-493, Also T.W. Hutchison, "The Significance and Basic Postulates of Economic Theory", op.cit.
52. The controversy took place mainly in the American Economic Review, 1964, pp.733-738, 1965, pp.1151-1172. For a discussion of this controversy see Bronfenbrenner, "A 'Middlebrow' Introduction . . .", op.cit. For more recent discussions see S. Wong "The 'F-Twist' and the Methodology of Samuelson", American Economic Review 1973, pp.312-24, A. Rosenberg, "Friedman's 'Methodology' for Economics: A Critical Examination", Philosophy of Social Science, 1972, pp.15-29. D. Pope and R. Pope, "Predictionists, Assumptionists and the Relatives of the Assumptionists", Australian Economic Papers, 1972, pp.225-228, Coddington, "Positive Economics", op.cit.
53. Friedman, "The Methodology . . .", op.cit., pp.14-15.
54. P.A. Samuelson, "Prof. Samuelson on Theory and Realism: Reply", American Economic Review, 1965, p.1165. Although both writers would use the term approximation to denote theoretical abstraction, the meaning attached to each case is different. Approximation for Friedman would mean such abstraction so as to produce "descriptively false", and heuristically true theories that generate valid predictions. Whereas for Samuelson approximation would mean such abstraction that would generate, on the one hand, simple and general theories, and on the other, would retain the true characteristics of empirical reality, i.e. would not be 'descriptively false'.

## FOOTNOTES (cont.)

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55. The following review relies mainly on Clarkson's "The Theory of Consumer Demand", op.cit., esp. pp.2-26, J. Melitz's "Friedman and Machlup On the Significance of Testing Economic Assumptions", Journal of Political Economy, 1965, pp.37-59, G.S. Massey's "Prof. Samuelson on Theory and Realism: Comment", American Economic Review, 1965, pp.1155-1163, and E. Nagel's "Assumptions in Economic Theory", American Economic Review, 1963, pp.211-219.
56. Although the review will not exhaust all the possible variations of distinctions and criteria, it will, nevertheless, suffice for the purposes of ordering and classifying the meanings and uses of the term 'assumption' in the F-Twist controversy. In addition it will only concentrate on the distinctions and criteria that correspond to the ones used by positivist philosophers. For example, distinctions such as those made between primitive and theoretical statements might seem, from a different point of view, artificial or naive. It could be claimed that a primitive or 'protocol' statement, although it describes empirical reality, is in itself cast in theoretical terms, or the empirical reality which it describes is a product of another theory. It should be remembered therefore that what follows is put into rather crude terms, and represents the general positivist position of a clear dichotomy between theory and fact.
57. See Clarkson, "The Theory of Consumer Demand", op.cit., pp.17-19.
58. See Clarkson, ibid., pp.19-21.
59. M. Friedman, "The Quantity Theory of Money: A Restatement", in his Studies in the Quantity Theory of Money, 1956, p.3-21.
60. Clarkson, ibid., p.5, and pp.20-21.
61. This distinction is made by Massey, "Prof. Samuelson on Theory and Realism", op.cit., p.1160.
62. See Massey, ibid., p.1160, and Melitz, "Friedman and Machlup . . .", op.cit., p.45.
63. However, the distinction between the two kinds of statements is finer and perhaps less clear than the one Positivists draw. A primitive sentence may contain terms that borrow theoretical terms indirectly, and perhaps unperceptibly, linked to experience. For instance, when one talks about the price of meat fluctuating say, with the frequency of industrial action in the mining industry, although prima facie it may seem a purely observational statement, nonetheless it contains concepts such as price which depend on, and need, theoretical corroboration in order to be defined. Given, therefore, that primitive statements can never be 'purely' un-theoretical, it must be added, however, that the difference between empirical-correlational hypotheses and primitive statements is one of degree rather than of kind. It is the distance between these two types of statements and theory that distinguishes between the two categories. A primitive statement is not as obviously and explicitly

(cont.)

## FOOTNOTES (cont.)

## PART I: CHAPTER 1:

63. (cont.) connected to a particular theory as is an empirical-correlational hypothesis. In addition, an empirical-correlational hypothesis may differ from a primitive statement in the degree of generality and scope of application.
64. Massy, ibid., p.1160.
65. Nagel, "Assumptions in Economic Theory", op.cit., p.215.
66. Nagel, ibid., p.212. As in the case of the two previous categories, theoretical statements and Fundamental Postulates are related. If one looks at the truth conditions and their relationship with empirical reality, one sees that both types of statement are qualitatively the same. The difference is one of degree and of status. Theories have a less distant correspondence with reality and they approximate it more closely than the Fundamental Postulates. The latter are the eschatological frontiers of a theoretical system. They are the assumptions behind any theory and the building blocks of any scientific paradigm. For the neoclassical system it is utilitarian liberalism with the accompaniment of 'rational economic man' that form the Fundamental Postulates. Theories of the firm and of the consumer are derived from them. These postulates approximate reality in an 'as if' manner and therefore are descriptively distant from it. The status of theories and Fundamental Postulates is different because the latter is the foundation upon which the neoclassical edifice is constructed and the theory is the frame that forms its shape.
67. J.K. Galbraith, The Affluent Society, 1958, pp.35-55, and Phelps-Brown, "The Underdevelopment of Economics", op.cit., pp.1-3.
68. Machlup, "Theories of the Firm", op.cit., pp.11-13.
69. R.M. Cyert and C.L. Hedrick, "Theory of the Firm: Past, Present and Future: An Interpretation", Journal of Economic Literature, 1972, pp.398-409.
70. Melitz, "Friedman and Machlup . . .", op.cit., pp.44-46. In this connection it is worth noting that Melitz shows inconsistency when he declares in p.51 that "Allowing that assumptions are often an important source of support of hypotheses, that they affect what is looked for and the interpretation of what is found, surely it is fundamentally the facts that support theories, rather than the other way around", and when he concludes in p.60 that, "The actual disconfirmatory significance of any negative test finding always depends largely on the individual circumstances involved; in particular, the character of the experiment, the quality of the execution and the nature of the results . . . In general, the potential impact of a disconfirmatory test result corresponds closely to the intensity of current reliance on the statement in question". In fact, this last statement about "intensity of current reliance" corresponds to the Kuhnian view of validity in terms of "acceptance" by the scientific community, rather than a strict observance of "the facts . . . support theories" criterion.

## FOOTNOTES (cont.)

## PART I: CHAPTER 1:

71. M. Weber, "Objectivity in Social Science and Social Policy", in Natanson's "Philosophy of the Social Sciences", op.cit., pp.392-402.
72. Robbins, "The Nature and Significance . . .", op.cit., pp.72-103.
73. Ibid., pp.88,104.
74. In fact, Nagel suggests this factor when he says, in connection to Friedman's essay on methodology, that "the essay is marked by an ambiguity that perhaps reflects an unresolved tension in his views on the status of economic theory" (my emphasis), "Assumptions in Economic Theory", op.cit., p.218. Also Massey notes that the "most significant feature of theoretical sentences . . . is that they are neither descriptively true nor descriptively false", "Prof. Samuelson on Theory and Realism . . .", op.cit., p.1162.
75. Translated by B. Jowett, 1970, p.148.
76. Friedman, "The Methodology . . .", op.cit., p.14.
77. This example about independent utility functions is interesting in that, although false, it helps Positive economists formulate the hypotheses generated from perfect competition in mathematical terms. It is interesting because of the positivist belief that mathematization is more important than relevance.
78. Melitz, for instance says that, "Friedman seems to argue that abstraction involves false allegation beyond the mere assertion that certain observable aspects are gone", "Friedman and Machlup . . .", op.cit., p.41.
79. Friedman, ibid., p.15.
80. The fact that he uses as an example of "assumption" the maximization-of-returns postulate, indicates that he means 'Fundamental Postulates'.
81. Friedman, ibid., p.15.
82. Friedman, ibid., p.39.
83. Ibid., p.26. It seems odd for Friedman to use the term "intuitive" when, in fact, in another article he, along with Meiselman, reproaches the Fiscalists for using intuition when specifying autonomous expenditures, by saying that they do not state "how different 'intuitions' are to be reconciled", M. Friedman and D. Meiselman, "Reply to Ando and Modigliani and De Prano and Mayer", American Economic Review, 1965, pp.783-784.
84. Ibid., p.30. Also Machlup claims that "we need not be very particular about the independent verification of the other intervening assumptions, the Assumed conditions, because judgement based on causal empiricism will suffice for them". "The Problem of Verification in Economics", op.cit., pp.17-18.

## FOOTNOTES (cont.)

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85. Friedman, ibid., p.22. Although this appears so for a Positive economist, a Marxian analysis would show that the survival of firms may be a manifestation of more fundamental (and perhaps irrational, i.e. contradictory) laws of the social structure. It is possible that firms survive because the social structure supports their existence, or that they react to fundamental contradictions such as the tendency of the rate of profit to fall, in which case it would mean that under a different social structure firms would not necessarily maximize. Friedman considers the firm in terms of a Schumpeterian entrepreneur filled with maximizing "animal spirits" rather than a firm situated within, and taking its particular characteristics from, a particular social structure. Moreover, the fact that a firm has gone bankrupt would not mean that it has failed to maximize its profits, but that it was squeezed out of the market due to the increasing degree of industrial concentration; and this may be the case even if the firm did maximize its own private returns.
86. See P. Winch, The Idea of a Social Science and its Relation to Philosophy, 1958, pp.127-8. Also Samuelson, while admitting the 'as if' nature of theories (cf. his "Maximum Principles in Analytical Economics", American Economic Review, 1972, p.250) also admits that "true minimization is what is important because the actors are postulated to have purpose from the beginning", ibid., p.258, and footnote 4 on p.257.
87. Samuelson, "Theory and Realism: Reply", American Economic Review, 1964, p.736.
88. Samuelson, ibid., p.736. See also reference in footnotes 93 and 94 below.
89. P.A. Samuelson. "Economics", op.cit., p.8.
90. Ibid., p.8.
91. Ibid., p.8.
92. Samuelson, "Prof. Samuelson . . .", op.cit., p.1168.
93. Samuelson, "Foundations . . .", op.cit., p.7.
94. Ibid., p.12 and p.22.
95. Ibid., p.22. See also "Maximum Principles . . .", op.cit., p.249.
96. Samuelson, "Foundations . . .", op.cit., p.23.
97. Samuelson, "Maximum Principles . . .", op.cit., p.250.
98. Samuelson, "Prof. Samuelson on Theory and Realism . . .", op.cit., p.1165.

## FOOTNOTES (cont.)

## PART I: CHAPTER 1:

99. Samuelson, "Prof. Samuelson on Theory and Realism . . .", op.cit., p.1168.
100. Ibid., p.1165.
101. It is interesting to note that even Knight reflects this ambiguity in his definition of Fundamental Postulates, see "What is Truth . . .", op.cit., p.158.
102. C. Ferguson, Microeconomic Theory, 1969, pp.5-6; Robbins is cited as an example of a priorism and Hutchison as an example of ultra-empiricism. See also Bronfenbrenner, "A 'Middlebrow' . . .", op.cit., pp.14-18.
103. Samuelson, "Prof. Samuelson on Theory and Realism . . .", op.cit., p.1166.
104. Friedman, "The Methodology . . .", op.cit., p.8.
105. Samuelson, "Economics", op.cit., p.9.
106. See J.G. Meek's Telling the Truth in Economics, 1976, unpublished Ph.D. thesis, University of Edinburgh.
107. An interesting example of a different explanation is M. Lazerowitz's psychoanalytic explanation of stalemate in perpetual philosophic conflict, in his Philosophy and Illusion, 1968, esp. pp.19-52 and pp.119-140.

CHAPTER 2

T H E   N A T U R E   O F  
E M P I R I C A L   E V I D E N C E

"For what can be imagin'd more tormenting than to seek with eagerness, what forever flies us; and seek for it in a place where 'tis impossible it can exist?"

David Hume<sup>1</sup>

## CHAPTER 2:

## THE NATURE OF EMPIRICAL EVIDENCE

## A. INTRODUCTION

In the preceding chapter I presented the structure of the methodology of Positive economics and focused on the place of theory in it. This led me to examine the nature of theory in Positive economics. The conclusion of this examination was that there are two fundamental interpretations of the nature of theory within Positive economics: one unrealistic and one realistic. A consequence of this division is, as we saw, the F-Twist controversy. The irresolution of the controversy led me to the observation that the conflict is due to an ambiguity in the meaning and use of the term 'Fundamental Postulate' or 'assumption'. I further hinted that this ambiguity is irreconcilable because it is dependent upon a fundamental contradiction structurally embedded in Positive economics. This contradiction arises out of the positivist split between, what I shall call in chapter 7, epistemology (the conception of the world) and ontology (the world itself).

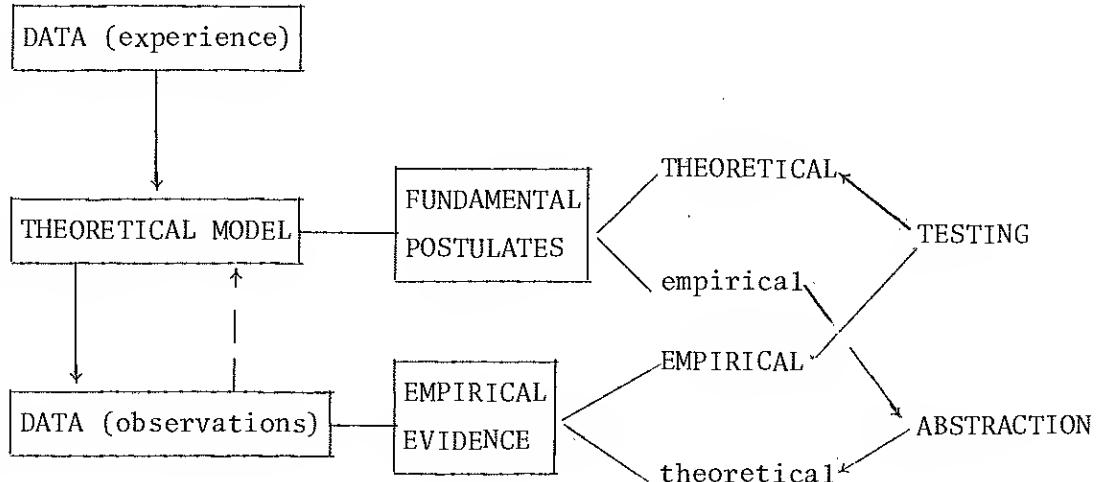
However, postponing further discussion on this issue until Part III, I shall now turn to examine the nature of the other half of the structure of Positive economics, namely empirical testing and evidence. In Figure I of the previous chapter, we saw that the upper part of the diagram shows the construction of theoretical models, while the lower part shows the place and role of empirical evidence. As mentioned in the above paragraph, Positive economists of both sides are characterised by an ambivalence in deciding whether the nature of

assumptions (behind models) should be purely theoretical or purely empirical. The general tendency is in favour of the theoretical with some anchorage in empirical reality. However, despite this agreement there are no precise criteria offered for determining the exact proportions of theory and experience in the formation of assumptions, with the consequent ambiguity producing rival interpretations. As we shall see in this chapter, this ambivalence or ambiguity characterises also the nature of empirical evidence, this time manifested in a reverse manner. In this case it is the empirical part of the dichotomy that plays the prominent role. Facts are mostly empirical but they need theories in order to acquire meaning. In addition, empirical evidence is couched within propositions that contain theoretical terms. For example the proposition that money lags behind income though referring to the empirical presupposes terms such as 'money' and 'income' the meaning of which depends upon an assumed theoretical paradigm. The exact proportions of experience and theory in empirical propositions are also ambiguous<sup>2</sup> and alternative interpretations give rise to controversies within Positive economics, such as for example the Monetary controversy. An important point to note is that in addition to the reversal of the ambiguity, empirical evidence differs from theory, according to Positive economics, in that it is supposed to reflect reality and thus to offer the sole criterion of accepting or rejecting a given theory. In other words, the status of empirical evidence vis à vis theories is assymetrical: empirical evidence tests theories but not the other way around.

The whole idea expounded above can be presented schematically.

In Figure II I have summarised the validational procedure shown in Figure I and I have connected it to the ambiguities arising from the dual nature of its terms. The left-hand side of the diagram represents the model of scientific inference in Positive economics, and the right-hand side shows the definitions (and ambiguities) of its parts. I have used capital and small letters to signify the relative prominence of 'empirical' and 'theoretical' in the structure of assumptions and empirical evidence. Furthermore, I have depicted the interrelations that exist between the two structures. The one manifested in the connection between 'EMPIRICAL' and 'THEORETICAL', with the former playing the leading role, and in the connection between 'empirical' and 'theoretical' with the latter playing the leading role. In other words, in the former case it is experience that renders a theory ultimately valid, whereas in the latter case it is the 'theoretical' that gives meaning to experience while the 'empirical' grounds theory to the earth.

FIGURE II: AMBIGUITIES IN THE STRUCTURE OF THEORY AND EMPIRICAL EVIDENCE IN POSITIVE ECONOMICS



Having therefore accounted for the 'theoretical' part of Positive economics, I now turn to examine the nature of the 'empirical' part. In doing this I shall first examine its justification, i.e. the philosophical problem of justifying induction. This examination will provide the context within which to discuss the ambiguities stemming from the definition of empirical evidence in Positive economics. In addition, it will provide some hints as to the fundamental contradictions in Positive economics. Furthermore, I shall discuss the positivist proposition of the independence between theory and evidence in a general, i.e. philosophic, and in a specific, i.e. Positive economic, way. Lastly, I shall comment on the question whether the representation of reality by empirical evidence is, firstly, logically sufficient and, secondly, actually reliable.

## B. INDUCTION

### a. THE PROBLEM OF INDUCTION

Perhaps the most relevant and fundamental question to ask concerning scientific validation in Positive economics is whether it is logically justified. We saw in the previous chapter that this procedure consists of testing economic theories with empirical evidence. In other words Positive economists are continuously confronting propositions containing theoretical terms with propositions containing empirical terms. They carry on this procedure by assuming that it is justified and permitted to draw valid inferences from empirical to theoretical propositions. But is it?

To some people this question might seem remotely distant from any practical and 'real' economic issues, and therefore irrelevant.

However, how can Positive economics claim rational (as opposed to metaphysical) status unless it has explicit criteria that render the produced economic knowledge logically valid? Perhaps one could argue that even though physics has not justified induction, nonetheless it does enjoy scientific status. But as we shall see, the Principle of Induction involves an assumption which stipulates uniformity in nature<sup>3</sup>, and at least prima facie, it seems that this assumption is truer in physics than in economics<sup>4</sup>. In consequence economics is faced not only with the problem of justifying induction, but also with the problem of justifying the legitimate application of induction on economic phenomena. Difficulty in using induction in economics is manifested in problems such as identification-specification and ceteris paribus clauses<sup>5</sup>. Additionally, if Positive economics is to claim that it is an empirical science, it needs more than others to justify induction. If it propounds that theories ought to be tested by empirical evidence, then it has to bring positive evidence to support this proposition. If it does then it has first to solve the Problem of Induction before it accepts the procedure. If it does not then it must accept the metaphysical nature of the proposition, since it is couched in 'ought' rather than in 'is' terms. But if metaphysical propositions are meaningless according to Positive economics, then obviously Positive economics must be itself meaningless. It seems therefore highly relevant and urgent for Positive economics to have a justification of induction. As Culbertson emphasizes, "Evidently our methodological problems bring us to head-on confrontation with the basic problem of empirical studies, the problem of justifying induction . . . Yet, this seems not to have been achieved."<sup>6</sup>.

The question therefore is important and relevant for any system of thought that attempts to acquire valid knowledge. As Hollis and Nell confirm, "we would . . . have to have a solution to the Inductive Problem without which no theory can be tested at all"<sup>7</sup>. Even more, it would not be an exaggeration to say that the Problem of Induction is important and relevant not only to science but also to everyday life. As Russell points out,

The general principles of science, such as the belief in the reign of law, and the belief that every event must have a cause, are as completely dependent upon the inductive principle as are the beliefs of daily life<sup>8</sup>.

The Inductive Principle refers to inferences that go from particular to general propositions. The Problem of Induction refers to the justification and validity of such inferences. If we define particular propositions as those propositions containing terms that relate to members of a given set of events and general propositions as those containing terms that relate to the whole, then the Problem of Induction can be stated thus: what guarantees that what holds for members of the set will also hold for the whole set? In the words of Hollis and Nell, "We must be justified in projecting the sample into the population"<sup>9</sup>.

The difficulty with justifying induction is in the kind of evidence that we seek in order to make the justification. If we seek empirical evidence we are obviously begging the question, because we are trying to justify induction with induction. If we seek theoretical evidence then we either refer to a general law proving induction or to a logical principle rendering induction self-evident. If, however, we

take the case of the general law as proof, then there is neither a priori evidence (as this would involve a petitio principi) nor empirical evidence (as this would involve the Problem of Induction) that guarantees that the general law will hold in the future<sup>10</sup>. If, in turn, we take the case that logical principles guarantee induction, then we find that there is also nothing to guarantee the truth or the self-evidence of logical principles. As Russell confirms, "induction is an independent logical principle, incapable of being inferred either from experience or from other logical principles"<sup>11</sup>. The consequence of this impasse is that since the Problem of Induction cannot be solved by a priori or empirical arguments, then we cannot know why we do what we do. As Joan Robinson appropriately puts it, "Why do I believe what I believe about what is that makes me believe it?"<sup>12</sup>.

This, however, creates problems for the Positive economists who want to be rational (i.e. to know and have reasons why what they do is valid) and test theories with empirical reality. To alleviate this burden therefore an implicit assumption is made in order to justify induction. This assumption relates to the uniformity of the world. By it we assume that occurrences in the past hold in the future because the world remains unchanged. Belief in this assumption involves a theoretical antecedent that states that "everything that has happened or will happen is an instance of some general law to which there are no exceptions"<sup>13</sup>. However, besides difficulties involved with the truth of the proposition itself, there are difficulties with trying to see if it is a kind of legitimate evidence that may justify induction. As previously, if the proposition is taken to be true according to empirical

evidence, then this creates an infinite regress as far as the Problem of Induction is concerned. If it is taken to be true a priori then another infinite regress is created since we would need another a priori proposition to support the initial one. If, on the other hand it is taken to be a synthetic a priori proposition, then it goes against the positivist position that analytic and synthetic propositions are clearly distinguished<sup>14</sup>. As far as the intrinsic validity or the self-evidence of the proposition is concerned, it is not at all clear how we can find criteria to say that because a law held in the past it would also hold in the future, because this would merely reassert the Problem of Induction. Moreover this belief involves more problems in the social sciences whereby the difficulty of finding immutable laws is notorious<sup>15</sup>. As Russell says,

The man who has fed the chicken every day throughout its life at last wrings its neck instead, showing that more refined views as to the uniformity of nature would have been useful to the chicken<sup>16</sup>.

Other approaches to the Problem of Induction, such as induction by enumeration or by elimination, also seem to have failed, because irrespective of how many instances one has found confirming a correlation, or how many disconfirmatory instances one has eliminated, there are neither logical nor empirical reasons to justify the generalization that the same association will occur in the future<sup>17</sup>.

It follows from the above that once we cannot know with certainty that what has occurred in the past will also occur in the future, the only way to hold such a belief is to attach to it, in

Keynes' words, "a rational belief of the appropriate degree"<sup>18</sup>, i.e. a probability. In other words, since we cannot know demonstratively that a conjunction of two events, or a theory incorporating it, will hold true in the future, we assign to it a probability or a likelihood, warranted by the evidence, that it will do so. Evidence of the kind that since, for example, in most instances it has been found that changes in money precede changes in income, then it is reasonable to argue that in all probability money will precede income in the next instance. However, in offering probability as a solution to the Problem of Induction there arise the following two difficulties:

Firstly, our belief that the frequency of a conjunction of events constitutes evidence for its probable recurrence, is a belief supported by evidence of the type that 'this method has been shown to work in the past'. This, however, being close to the pragmatist solution, introduces a circularity in the argument that prevents it from being used as a solution to the Inductive Problem. Therefore, and this is the second difficulty, since this belief cannot be inductive it has to be deductive, i.e. theoretical. As Strawson puts it, "It is an analytic proposition that it is reasonable to have a degree of belief in a statement, which is proportional to the strength of the evidence in its favour"<sup>19</sup>. But this reduces the justification of induction to deduction, and as Braithwaite says, "The doubt as to whether we have any reason to rely upon inductive procedure . . . is not a sensible doubt to be allayed by postulating a 'supreme major premise'"<sup>20</sup>. To assimilate induction to deduction, however, requires the justification of the certainty and validity of the deduction, in which case one could bring neither inductive nor deductive evidence. It seems therefore that if the idea of

probability cannot be supported by empirical evidence, because of the Problem of Induction, then we have to turn to a theoretical assumption that might provide some fundamental certainty from which to start off<sup>21</sup>.

According to Barker,

Unless something is certain, nothing can be probable. There must somewhere be a bedrock foundation upon which we can build. Unless some basic evidence is certain, we shall have at best only a web of relative probabilities hanging in air, insufficient to establish that any hypothesis really is rationally credible.<sup>22</sup>

Keynes' purpose in his "Treatise on Probability" was "to give prior probabilities that would justify inductive inference"<sup>23</sup>. That is, Keynes attempted to establish a logical relationship between empirical and theoretical propositions that is embedded in human rationality<sup>24</sup>.

It follows, therefore, that a statement involving probabilities is based upon an assumption which turns out to be theoretical<sup>25</sup>. Accordingly, if we were asked to justify the truth of this assumption, we would have to find criteria that would be able to solve the regress created were we to justify it by further assumptions<sup>26</sup>. Thus it seems that probability presupposes the Problem of Induction<sup>27</sup>. As Keynes himself in the end admits,

In my judgement, the practical usefulness of those modes of inference, here termed universal and statistical induction, on the validity of which the boasted knowledge of modern science depends, can only exist - and I do not now pause to inquire again whether such an argument must be circular - if the universe of phenomena does in fact present those peculiar characteristics of atomism and limited variety.<sup>28</sup>

Thus since probability involves a priori justification, it cannot be accepted by the thorough-going empiricist who must justify induction

by induction, unless he accepts the validatory powers of theory and logic.

It would take at least one more thesis if I were to continue reviewing all the possible solutions that have been attempted in order to tackle the Problem of Induction. Different people in different ways have offered different solutions to the puzzle<sup>29</sup>. Indicative of the confusion and ambiguity created by the Problem of Induction and the variety of solutions attempted is the following quotation by Lazerowitz, who having reviewed the various positions concludes that,

Some practiced philosophical thinkers are convinced that there is a problem of justifying induction . . . Other practiced thinkers are equally certain that there is a problem . . . while others think it is unreasonable to ask for . . . a justification of induction . . . Still other philosophers are undecided about whether there is a problem . . . some think it has not yet been solved. Others are persuaded that it has been solved, but they are not decided on what the solution is. 30

Sufficient however for my purposes in this section is to indicate that the problem is (a) fundamental, and (b) if not unsolvable, at least ambiguous. Firstly, it is fundamental because it presupposes the rationality, i.e. the logical justification, of any system of thought, and secondly, it is ambiguous because it eludes any type of justification. The consequence of these two characteristics of the problem is a contradiction facing a system of beliefs, such as Positive economics, that wants to be rational and yet cannot find a rational way for justifying why it believes what it believes. This contradiction, as we shall see in chapter 7, generates problems, such as rival definitions of what has to be theory and evidence, that have

to be solved if the system is to be at all viable. The 'solutions' to these problems - which I would rather call mediations<sup>31</sup>, because they are not unambiguous solutions but only concessions (conscious or unconscious) to the contradiction; hybrid forms of terms involved in the contradiction, that enable the system to establish itself as viable - form the building-blocks of the entire edifice of any system of beliefs. However, as these building-blocks are ambiguously formed, they offer themselves to ambiguous interpretations and thus to polar views and unsolvable conflict.

Finally, the ambiguity in the justification of the Principle of Induction lends itself to problems regarding the nature of evidence and particularly the nature of empirical evidence. That is, there are difficulties in being able to assign the status of valid evidence to a particular proposition, and difficulties, in the case of empirical evidence, to decide what constitutes observations of empirical phenomena and what does not. In what follows I shall try to review some of these problems, relate them to the Problem of Induction and then, in the following section, see how they affect Positive economics.

#### b. EVIDENCE AND OBSERVATION

If we assume, for a moment, that the Problem of Induction has been solved and that we are now justified to make inductive inferences, then the question arises as to what we can legitimately call inductive and what not. In other words what logical criteria are there to tell us how to discriminate between propositions that are purely empirical and propositions that are purely theoretical?

As far as the positivist syndrome, well placed within the physical sciences and economics, is concerned, undoubtedly there are such criteria. The empirical should relate to the observable and the theoretical to the unobservable. According to Carnap the language of science is divided into two parts:

The observation language and the theoretical language [where] the observation language uses terms designating observable properties for the description of observable things and events [and] the theoretical language [uses] terms which may refer to unobservable events . . .<sup>32</sup>.

Observation of an object is possible through our sense experience of the properties of the object. In a sense the object emanates data of which we become aware through our senses, while the object itself remains unknown<sup>33</sup>. According to Russell awareness of these sense data is direct and does not involve any inference. In other words, we do not need any previous knowledge or any intermediate process with which to become acquainted with sense data<sup>34</sup>. In short, our awareness of sense data can be neither true nor false, it is simply there<sup>35</sup>.

However, is the perception of sense data the same as the observation of an object? According to Ryle we must make a conceptual distinction between sensation and observation<sup>36</sup>. Sensations entail a direct awareness of the epiphenomenal properties of an object, which comes to us by experiencing it, whereas observations entail some kind of question we have put forth about something which we want to find out, thus involving previous judgements implying truth and falsity. As Ryle suggests, "to be observing something the observer must also at least be trying to find something out"<sup>37</sup>. In addition observation

differs from sensation in that it also involves description. When we are observing an object we are describing its properties or its external behaviour. If this is so, then, according to Russell, "Knowledge of things by description . . . always involves . . . some knowledge of truths as its source and ground"<sup>38</sup>. In consequence, if observations imply (a) a judgement of that about which we are making our observations<sup>39</sup>, and (b) a judgement of the truth of a priori statements needed for describing a state of affairs, then it is very difficult to support the claim that propositions containing observational terms are distinct from propositions containing theoretical terms, let alone support the claim that the former propositions test the latter<sup>40</sup>.

In view of such difficulties many proposals have been put forth offering criteria that could enable one to distinguish between observables and unobservables. For instance, Spector in reviewing some of these proposals, quotes Hempel who brings forth as a criterion of valid observation the "degree of agreement . . . different observers, by means of direct observation" can assign to their "observational vocabulary" which includes "subjective impressions, inter-subjective sense-data and finally gross physical objects and their properties"<sup>41</sup>. No doubt many other criteria could be proposed in terms of which the independence between theory and observation could be safeguarded<sup>42</sup>. However, according to the critics, most of these criteria seem unsatisfactory. Feyerabend, for instance, does not ascribe any "special content" to observational statements. He claims that,

If there is a difference between them and other statements, then this difference is provided by the psychological or physiological or physical circumstances of their production.<sup>43</sup>

Or Hayek claims that "the properties possessed by the concrete objects" do not necessarily belong to these objects, but they are created by the classificatory function of the brain<sup>44</sup>. The point of these criticisms is that the objectivity designated to empirical observations is weakened by the fact that the process of making observations is inferential and derivative, and thus includes judgements and subjectivity. According to Barker,

The whole notion of direct observation is an extremely hazy one. In the nature of the case, it will be impossible to draw any sharp and definite distinction . . . between what is and what is not directly observed; insofar as a distinction is so drawn as to be plausible, it will be indefinite; and to make it more definite would be to draw a line that would be implausibly artificial and arbitrary.<sup>45</sup>

Consequently, the much needed independence between theories and observations for the positivist validation procedure is broken. What remains is a dependence which prevents empirical observations from being used as evidence for the validation of theories. However, someone could argue, as Nagel does, that although empirical observations are dependent on theories this does not mean that a particular theory should be included among the set of theories upon which observations are supposed to depend<sup>46</sup>. This criterion, however, leaves much to be desired since, firstly, it does not tell us how we shall know whether the particular theory to be tested is or is not among the theories that determine the observations, and secondly, according to Kuhn's

theory of the paradigmatic nature of science, it is difficult to separate a theory from the observations, since the latter are perceived from the point of view of the paradigm that furnish the theory in question. Thus Nagel's criterion cannot be sustained as a valid criterion for separating between theory and observation.

This brief review of the difficulties encountered in empirical observation was not meant to be exhaustive and offer a comprehensive account of the different stands in this controversy<sup>47</sup>, but merely to point out that empirical observation, and hence empirical evidence, is not a straightforward matter, as Positive economists present it, but one which involves many problems. These problems coupled with the ambiguities due to the Problem of Induction, contribute directly to the explanation of conflict in Positive economics. In fact the theory-fact problem seems to be directly linked to the Inductive Problem. That is, an empirical proposition containing observational terms needs something other than another empirical proposition to support its logical status and validity. It needs a proposition that would include a priori judgements and thus theory. Hence, logically, empirical observation is dependent on theory, and the independent status of empirical evidence is put into question<sup>48</sup>. In assessing the relation of these problems to conflict in economics I shall examine how these difficulties affect the structure of Positive economics and produce inherent tensions.

## C. INDUCTION AND POSITIVE ECONOMICS

## a. THE PROBLEM OF JUSTIFYING INDUCTION IN POSITIVE ECONOMICS

Even if Positive economists are not explicitly aware of the existence of the difficulties created by the Problem of Induction, the assumption of rationality behind the testing procedure they use obliges them to justify the choice of such a procedure. The most common and widely favoured justification is the pragmatist one. This justification rationalizes the procedure by stating that the success of the Principle of Induction in the past is a sufficient criterion for its success in the future<sup>49</sup>. Since, in other words, the correlation between money and income is indeed found to project well in the future, this proves that induction works well. If a theory has predicted well in the past it will also predict well in the future, thus justifying induction. Success of the predictions of an hypothesis with observed phenomena increases the probability of its success with unobserved ones, and this makes the hypothesis a more preferable one. The important assumption for Friedman is "whether the theory works, which means whether it yields sufficiently accurate predictions"<sup>50</sup>.

However, there are two logical points that seem to go against this position. Firstly, if we assume that a theory is successful because it works, then we cannot produce any argument to sustain the belief that it will go on working. We cannot do this because if we brought more empirical evidence to support the hypothesis, that would not be more than blatantly repeating the problem afresh. The hypothesis works well because it works well. But what logical assurance do we have that it will go on working well? The answer is:

let us wait and see. If the hypothesis does not work well for unobserved cases then we shall reject it and choose another one. But the answer really evades the question because even if the hypothesis works well in the future there is nothing to guarantee that in a fresh instance it would go on working. The confirmation of the hypothesis does not render the hypothesis valid<sup>51</sup>. Secondly, the procedure itself, i.e. testing hypotheses by confirming their predictions with experience, cannot be justified by saying that it has worked in the past because this would mean that the pragmatist position is justified by appealing again to the pragmatist position<sup>52</sup>. It seems therefore, that the belief in testing hypotheses with predictions is nothing else but a faithful expectation, with neither logical nor empirical criteria to support it<sup>53</sup>. A contradictory position it seems for someone such as a Positive economist who does not accept the validity of faith and metaphysics.

Furthermore, if the Positive economist is pressed to defend the argument, he/she would have to turn to a position admitting theories the role of the guarantor of future predictive capacity. Thus Lipsey says,

Some sequence of events, some regularity between two or more things is observed in the real world and someone asks why this should be so. A theory attempts to explain why . . . a theory enables us to predict as yet unobserved events. <sup>54</sup>

But what does this position amount to if we take into consideration that, firstly, a theory cannot justify induction as this would imply that the Deductive riddle has been solved, i.e. a better theory was

found to validate the initial theory, and secondly, that if inductive criteria are insufficient to validate a theory (due to the Problem of Induction), then how can we choose between alternative theories, when in fact Positive economics describes theories as tautologous and empty, acquiring meaning only through their application to experience<sup>55</sup>? Any simple and logically consistent theory can be as good as any other since by being labelled tautologous, its terms can be replaced by another set of terms without altering the tautologous relationship. For instance, if we cannot be justified in supporting the Quantity Theory of Money on the basis of its predictive capacity, since we do not know why we should do this, then the relationship  $MV=PT$  can be replaced by any other tautologous relationship such as  $AB=\frac{AB \cdot B}{B}$  without making any difference whatsoever. The upshot of the argument is that once a Positive economist thinks of theories as empty he cannot use them as guarantors of the continuation of a given conjunction of events into the future<sup>56</sup>.

Similarly, the two arguments implied in the positivist position to the effect that a hypothesis is validated either by confirming its predictions with experience or by succeeding in not being disconfirmed by counter evidence, seem also to impose the Problem of Induction anew, without coming closer to a solution. If by confirmation is meant the number of successes a hypothesis has had in making correct predictions, then the mere quantitative addition of confirmatory instances will not alter the qualitative nature of the problem. That is, even if we have 100 per cent of the cases for a particular hypothesis being confirmed, this will not guarantee its confirmation in

the future<sup>57</sup>. The Phillips curve is a case in point whereby although Phillips, Lipsey and other empirical researchers found instances confirming the hypothesis, it proved that in the next instance the inverse conjunction of price and unemployment rates did not hold<sup>58</sup>. If, on the other hand, by confirmation is meant failure of the hypothesis to be contradicted<sup>59</sup>, in which case the hypothesis is tentative and subject to continuous testing, then a different problem arises. According to this position, taken after Popper, a hypothesis is scientifically validated, if it is falsifiable. This methodological assumption is quite popular among Positive economists who claim that "Positive economics . . . deals with statements that could conceivably be shown to be wrong (i.e. falsified) by actual observations of the world"<sup>60</sup>. Although Popper's theory of falsification does overcome the problems of confirmation, it does not however provide a satisfactory solution to the Inductive Problem<sup>61</sup>. The Problem of Induction arises because confirmation does not guarantee the re-occurrence of the hypothesis, whereas falsification implies that the non-recurrence of the hypothesis is evidence for its refutation. However, the occurrence or the non-recurrence of an hypothesis is calibrated in terms of propositions that contain empirical observations of events. Popper in fact modifies the vulgar empiricist tradition and accepts the theory dependence of empirical observations. However, despite this modification he is still faced with the problem of justifying the validity of these propositions containing the observations. Why should a particular observational proposition claiming the non-recurrence of an hypothesis be more valid than one opposing it? Clearly, to invoke theory as a selection criterion would be circular for Popper who

considers theories as refutable conjectures. Also to invoke another observational proposition would be again circular, since the Problem of Induction is reasserted. Thus, in fact, Popper's solution of the Problem of Induction presupposes it in a circular and arbitrary fashion.<sup>62</sup>.

Thus, if the above are true and confirmation and falsification are logically unattainable, then Positive economists find themselves in a limbo since they cannot justify the rationality of the methodology that in fact expounds rationality as the fundamental building block of scientific knowledge. The contradiction is, of course, latent and manifests itself in the rationalizations that Positive economists make in order to cover it. For instance, Positive economists try to hold on the distinction that separates analytic or theoretical from synthetic or empirical propositions. They claim that because of this independence and because empirical evidence is more trustworthy than theories - as it is the outcome of observing reality - the former ought to test the latter. However, as we shall see, due to the Problem of Induction, this distinction is not clear, and in practice (i.e. in the definition Positive economists give of empirical evidence) mixed ideas mediate for the contradiction implied in designating empirical evidence as a reliable and justified criterion of validation.

Thus, there arise two questions: firstly, do Positive economists in theory and practice follow the prescribed dichotomy between theory and fact, and secondly, irrespective of the position taken by Positive economists, can this dichotomy be sustained in economics? In the following section I shall attempt to comment on these questions.

b. EMPIRICAL EVIDENCE AND POSITIVE ECONOMISTS

The methodological premise of distinguishing analytic from synthetic statements is important because it renders empirical testing and Positive economics according to Coddington's account of it, "a consequential activity, i.e. an activity having definite and unambiguous implications for the theory in question"<sup>63</sup>. It seems therefore essential for Positive economists, (a) to define the terms analytic and synthetic, and (b) to show that the distinction holds true. Having seen in chapter 1 that the analytic part of the distinction has not been unambiguously defined, I now turn to examine whether the synthetic part is clearly postulated.

What do Positive economists mean by a synthetic proposition? According to Coddington's account of Positive economics, it is "something which is variously referred to as 'facts', 'evidence', 'experience' or 'observation'"<sup>64</sup>. All these terms serve to denote reality against which theories ought to be tested. The questions that arise, however, from such a definition of reality are: (i) do the terms represent reality, and (ii) do Positive economists use these terms unambiguously? The question whether empirical facts represent reality will be tackled in section C within the context of a Marxist critique of empiricism. Also the question whether empirical facts represent reality reliably will be tackled in section D. But first I shall discuss the question of the ambiguity of empirical evidence.

The strong aura of empiricism that pervades Positive economics suggests that synthetic propositions contain pure empirical facts that

are independent of the theoretician using them. When Positive economists proclaim the independence between theory and facts they do that because it is essential for the validity of empirical testing. However, are they unequivocal with this distinction when they define 'empirical' and relate it to the 'theoretical'? From their methodological writings it seems they are not. For instance, although they would claim that theory is "a set of tautologies" or "analytical filing system" and that "Factual evidence alone can show whether the categories of 'analytical filing system' have a meaningful empirical counterpart"<sup>65</sup>, thus implying the independence between the two, they would also try to qualify it by saying that "A theory is the way we perceive 'facts', and we cannot perceive 'facts' without a theory"<sup>66</sup>. Another example of this inconsistency comes from Samuelson who, on the one hand argues that "Every science is based squarely on induction - on observation of empirical facts . . . deduction has the modest linguistic role of translating certain empirical hypotheses into their 'logical equivalents'"<sup>67</sup>, while on the other hand he argues that "how we perceive the observed facts depends on the theoretical spectacles we wear . . . To a degree we are prisoners of our theoretical preconceptions"<sup>68</sup> (my emphasis).

But are these qualifications tenable for Positive economists who cannot afford any sort of interdependence once the consequential activity of testing is propounded? Either empirical facts are theory-free and may test theories, or they are theory-laden and may not test theories. Therefore it seems that Positive economists are inconsistent in claiming the methodological validity of the empirical testing of theories and the independence between theory and fact, which is needed

for that methodology, and yet acknowledging the necessity of using theories to interpret facts<sup>69</sup>. Nonetheless, this inconsistency is mediated by the positivist idea that theory plays a very small, linguistic-analytical, role. Thus, Positive economists in defining facts and their relation to theory, produce a structure that is the reverse of the structure of assumptions, as defined in the previous chapter. Whereas assumptions are mostly theoretical with some empirical correspondence, facts are considered to be mostly empirical with some connection to theory (thus the emphasis in 'degree' in Samuelson's quotation; see Figure II on p.65). As with the definition of assumptions so with the definition of facts, there is an ambiguity stemming from this inconsistency. The ambiguity refers to the lack of criteria for determining the precise content and proportion of the EMPIRICAL-theoretical mixture (see Figure II) in the definition of empirical facts. How much theoretical should the interpretation of empirical facts be? The consequence of this ambiguity is to contribute to the irresolution of the Monetary controversy<sup>70</sup>. As we shall see in chapter 6, the conflict goes on, among other reasons, because there are no precise criteria for determining the extent of the theoretical in empirical evidence. With every different definition of the specification of the variables in the models used, the empirical world appears differently. For instance, the Andersen-Jordan tests have been corrected and respecified by their opponents on the basis of different definitions producing different empirical results<sup>71</sup>. Also the direction of the lag in the notorious money-income correlation has been contested on the basis of a different theory postulating a different relationship and direction<sup>72</sup>. Thus, empirical facts test theories but

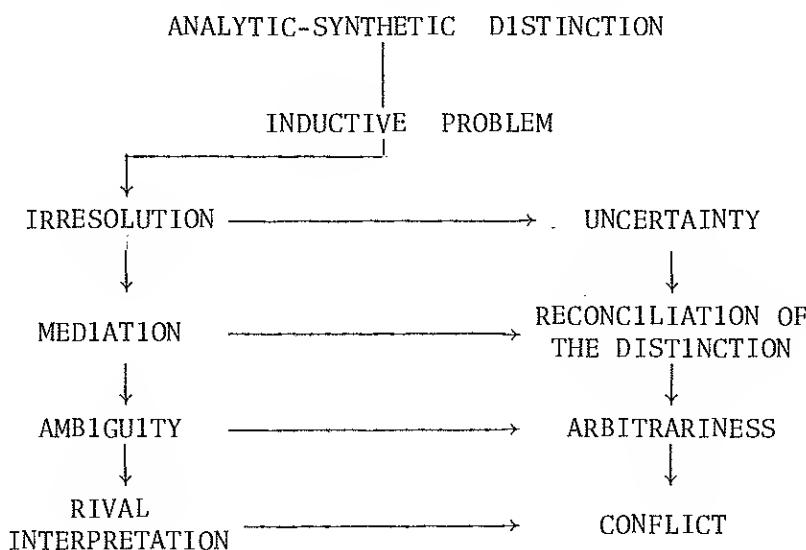
once some dependence is acknowledged then the ground is open for rival interpretations of facts.

However, why do Positive economists feel the need to modify or qualify the absolute independence between theory and fact? Had they postulated a purely 'theoretical' theory and a purely 'empirical' fact, Positive economics and empirical testing would have logically, but perhaps not practically (due to problems of data reliability and reality representation, to be discussed below), worked perfectly, and persistent controversies might have been resolved. In my opinion the modification of the independence assumption is necessary because of the existence of the Problem of Induction. How could Friedman, or any other Positive economist, justify his belief that the money-income correlation would hold in the future because it held in the past, unless a theory was provided, in his case the restatement of the Quantity Theory of Money, that would rationalize the occurrence and thus render the recurrence of the conjunction theoretically plausible? According to Friedman, "Every empirical study rests on a theoretical framework . . . That framework is the quantity theory of money"<sup>73</sup> (an empirical study being a potential empirical fact). Ideally a Positive economist would want science to work in terms of a perfect dichotomy between theory and fact, with the latter testing the former. But the uncertainty that springs from making generalizations from the particular necessitates the introduction of theory. The question is how much and which theory? Once a posteriori criteria of prediction are not logically valid, as this would beg the Induction Riddle, a priori criteria become necessary. But for the Positive

economist a priori criteria for determining the degree and choice of theory are meaningless unless accompanied by empirical confirmation. However, since the latter cannot be done, as it would repeat the process, then an element of arbitrariness is inserted in the testing procedure of Positive economics. The consequence is unresolved conflict. If Positive economics postulated a different epistemological paradigm embracing a different role for theory then, to a certain extent, the arbitrariness might have been resolved. But then that paradigm would not have been Positive economics.

The crux of the argument presented in this section is that unresolved conflict in economics emerges from an ambiguity in the definition of facts, that arises from an inconsistency due to the irresolution of the Inductive Problem. In Figure III I have depicted the argument diagrammatically so as to present, in a step-by-step manner, the structured contradiction within Positive economics<sup>74</sup>.

FIGURE III: AMBIGUITY AND EMPIRICAL TESTING



Once the ideal methodological distinction is postulated, Positive economics is faced with a riddle which needs solution if the methodology is to be logically justified. However, the irresolution and the elusive character of the riddle necessitates the logical reconciliation of the distinction. The consequence is a definition of 'synthetic' in terms of a hybrid form containing some 'theoretical' and some 'empirical'. The ambiguity implied in this definition of empirical evidence results in tension and unresolved conflict<sup>75</sup>. As we shall see in Part II the Monetary controversy is an example of such a conflict. While everyone in the controversy agrees as to the nature of the test for choosing between the two theories, there is widespread disagreement as to the nature of the evidence that does the testing. The form that empirical evidence takes corresponds to different theoretical conceptions, definitions and specifications.

### c. THE INDEPENDENCE AND REALITY OF EMPIRICAL EVIDENCE

Having answered the first question asked in the end of section C.a., I now turn to the examination of the second one. Assuming, for the moment, that the analytic-synthetic distinction does not encounter all these logical difficulties and ambiguities and that it holds true logically and unambiguously, the question then is, does it in fact hold true in practice, as far as economics is concerned? In what follows I shall try to examine the actual problems facing the distinction in cases such as the identification-specification problem and the use of ceteris paribus clauses.

As we have seen, the methodology of Positive economics assigns to theory a vacuous role. Theories are supposed to be figments of the imagination of the theorist that become 'real' once they are tested with the facts. But what are the facts? How is reality defined according to Positive economics? Reality according to the latter is defined by the empirical world immediately observable to us. The only thing real is empirical data. Theories are only "filing systems". From this Positive economists conclude that since reality is empirical facts and theory is independent of it, then empirical facts should test theories. Thus the important assumptions are, (a) that empirical facts can be independent of theory, and (b) that empirical facts represent reality.

However, besides the logical difficulties we have already encountered concerning the independence of fact from theory, there are also actual difficulties that are related to the identification problem. Firstly, when we collect a set of data that are supposed to relate to a particular section of the empirical world, how do we know that this set of data in fact relates to this section? And secondly, when we are looking for a particular pattern in the empirical world how can we distinguish between the variety of forces that cause this pattern?

Regarding the first difficulty, when we have a collection of data that are supposed to reflect macroeconomic behaviour, how do we know that a particular series of numbers represents income or expenditure, investment or savings? This knowledge cannot be given

by the facts because these are presumed to be the facts. What we need, therefore, is some previous grounds to lend support to our belief that these data can be legitimately grouped into the categories of income, expenditure, savings or investment. In other words, the empirical world is delineated with the help of theories. "Theory indeed", Hollis and Nell say, "even plays a part in determining what are the facts to be considered"<sup>76</sup>, and if this is so then in practice the independence of empirical evidence cannot hold true.

Regarding the second difficulty, Positive economists are faced with harder problems. Suppose we wanted to measure a savings and investment relationship or a money and interest relationship. Then, we would confront the classic problem of identification, whereby a scatter diagram of these relationships would not tell us whether what we are looking at is a savings or investment function or, in fact, both<sup>77</sup>. If in a system savings are dependent on income and interest rates and investments are dependent on the same determinants, then solution to this system would produce points in a diagram that would represent equivalently a savings or investment or a 'mongrel' function. What is needed, and what is done in practice, in order to separate these functions is some additional specification postulating a greater variability of one function relatively to the other. The inclusion of additional variables that seem to be a priori significant also contribute to the identification of the relationship. Other restrictions such as specification of random disturbances or non-linearities are important identification conditions but they are also dependent on theory. As Klein confirms,

In effect, this is an a priori restriction on the specification of the model. If we have reason to believe that one disturbance is more variable than another . . . then we have an identifying restriction on the system.<sup>78</sup>

There is no doubt that facts do emerge, but they are dependent on theory. But someone could argue that this is only a conceptual problem and in practice the theory-fact distinction can be maintained. But, as Klein observes, identification is an important problem and it is not only conceptual but "a definite problem in the interpretation of equations computed from observations which are themselves the outcome of the mutual interplay of such equations in the market process"<sup>79</sup>. An argument could be that the a priori theorizing necessary for identification can be tested a posteriori with the application of the model. However, as we have seen, this argument cannot be supported since there must be a choice of theory before the model is tested. Moreover, the application of the model implies a set of data upon which it applies, in which case the identification problem reappears.

Another actual difficulty that vitiates the theory-fact distinction relates to ceteris paribus clauses<sup>80</sup>. This time-honoured difficulty arises because in an ever changing, complex world we cannot distinguish 'economic' from 'psychological', 'physical' or any other kind of facts; or decide between significant and insignificant influences, unless a previous theory gives us good reasons to support the belief that some forces are held constant while others vary. Thus again theory plays a fundamental role in rendering social facts distinguishable and in guiding the observation of the flux of social phenomena. But if this is so then facts are theory impregnated and

thus illegitimate for Positive economics. Thus, this means that empirical facts cannot test theories, as they are theory laden. And this also applies to neoclassical theory. But then why do Positive economists use Neoclassical economics, as opposed to any rival theory, as a perspective that distinguishes relevant from irrelevant forces? Moreover, if the neoclassical spectacles order the observable world into the relevant and economic, then one cannot use the latter to test the former, as it is dependent on it. Also the 'neoclassically' observed facts cannot test an alternative theory offering a new perspective, since they will always be biased in favour of the neoclassical perspective.

Since, therefore, both logical and actual problems impede the application of the theory-fact distinction, it cannot be maintained that the activity of testing theories with empirical evidence can be deemed as 'consequential'. I emphasize actual in order to distinguish it from the logical difficulties presented in the previous section that referred to the Inductive Problem. However, the two types of difficulties are in fact related. The actual problems facing the analytic-synthetic distinction are nothing else but manifestations of the logical ones. If the logical problems stem from the irresolution of the Problem of Induction, then the actual problems are a repetition of the Problem in a changed form. In the case of identification and ceteris paribus clauses, the Problem asserts itself in terms of a difficulty in justifying with more empirical facts the determination of empirical facts. If we cannot know how to distinguish between a demand and a supply function in a set of empirical data, we cannot bring more empirical data to help

us as this would merely double the difficulty. As in the logical case, what Positive economics needs is a theory to render the data plausible, comprehensible and relevant. However, since facts are theory-laden, empirical testing cannot be used to select between alternative theories, and thus Positive economics is untenable.

As far as the second assumption is concerned, i.e. that empirical facts represent reality, it seems that it is an assumption that in the face of criticism from alternative epistemologies cannot be sustained. Empirical facts play a 'consequential' role in theory testing for Positive economics, because they are supposed to represent reality. The logic behind this assumption is in congruence with the wider positivist principles which define reality as that which can be observed<sup>81</sup>. Anything that cannot be observed and cannot be rendered in observational language is thus labelled metaphysical and so 'unreal'. Entities such as 'purpose', 'consciousness', 'essence', belong to the domain of the unobserved and therefore to the unreal. Positive economists trust empirical observation because it is in this way that concrete reality is perceived. Thus man's behaviour cannot be defined according to what goes behind this behaviour (e.g. consciousness) because the latter cannot be seen and commonly observed. Hence, positivist theories of man depend on external behaviour and its patterns or regularities. This also applies to economic behaviour. The determinants of economic phenomena are not defined in terms of 'hidden' or 'inner' properties that cannot be ascertained in any way other than speculation. But they are defined in terms of their external properties. Accordingly, the unit of economic measurement is not value but price as

it is formed in the market. Economic quantities and magnitudes are determined not by any invisible process or structure but by such 'visible' (i.e. quantifiable) units as supply and demand, that ultimately depend on prices. Even such processes as 'subjective utility' or social welfare are looked at with suspicion, and 'revealed preferences' or social costs and benefits take their place. The latter are not considered metaphysical because they can be measured with price. Thus price is the focal concept which is determined in the market, and the market is a phenomenon the behaviour of which can be commonly observed.

However, the definition of reality in terms of empirical observables constitutes an assumption based upon the belief that the phenomenal is identical to the real. And this belief, in turn, is borne within an epistemology that propounds sense-observations as the predominant feature of reality<sup>82</sup>. But what is the justification of this assumption? Why should one rely on sense-observations and empirical measurements? Why should the phenomenal be more real than what is behind phenomena? If what is behind phenomena cannot be observed with the 'naked eye' that does not mean that its existence cannot be ascertained through abstraction and logic. And if Positive economics justifies this assumption because it has worked for Physics, it cannot claim that it has also worked for economics. The abundance of unresolved controversies, continuous disagreements and the ill-fame of economics testifies to the opposite of this justification<sup>83</sup>. Consequently, the assumption of the supremacy of empirical evidence in representing best reality is a belief the justification of which depends

on a logic that is defined within a philosophical school, namely Positivism<sup>84</sup>. But to see whether this logic is sufficient for justifying the assumption we have to examine an epistemology that propounds an opposite logic, a logic, that is, that accepts non-observable entities as real, namely Marxism. This logic, as we shall see, also contains a critique of this empiricist theory of reality.

Reality, according to Marxism, is a process<sup>85</sup>. It is a historical-dialectic movement that requires a structural analysis rather than only an epiphenomenal one<sup>86</sup>. Marxism assumes a multi-dimensional reality the various levels of which are interconnected and dialectically opposed. Thus, sense-perceptions constitute one level of reality that either opposes (or falsifies) another deeper level, or simply reflects it. However, for Marxism, sense-perceptions are not the level of reality that determines all others, simply because these sense-perceptions are objects that are dissolvable to more essential and thus unobservable entities. As Marx says,

The concrete is concrete because it is the concentration of many determinations, hence unity of the diverse. It appears in the process of thinking, as a process of concentration, as result, not as a point of departure, even though it is the point of departure in reality and hence also the point of departure of observation and conception. 87

It seems, therefore, that Marxism accepts the appearance of reality in terms of observables, but defines its determinants in terms of unobservables. In consequence, the method of reaching this 'essential', unobserved, reality is "only through the criticism and reconstruction of sense-perceptions by logical reasoning"<sup>88</sup>. However, sometimes reality

appears to the observer so as to conceal the underlying level of reality. The observer thus perceives the appearances as real, when in fact they may falsify reality. According to Godelier, "it is not the subject who deceives himself, but reality which deceives him"<sup>89</sup>. But not all appearances are deceptive; processes that go on the surface of reality may be a reflection of a deeper reality. For instance the circulation of commodities is a process that reflects and is determined by the process of production that goes underneath it. The methodology for Marxism is to understand the surface processes, analyse them, find their determinants and then logically reconstitute them. In this the total structure of reality is understood rather than only a small part of it. And this is where the critique of empiricism becomes relevant.

Since Positive economics directs its observations to sense-perceptions (i.e. measurable events), which constitute appearances, it represents either an insufficient or a distorted outlook of reality. "Vulgar economy", as Marx argues, "sticks to appearances in opposition to the law which regulates and explains them"<sup>90</sup>. In effect, therefore, economic reality is composed of two levels: of a surface reality pertaining to circulation and exchange, i.e. the market, and an inner reality pertaining to a set of production relations, i.e. capital versus labour. Thus, "exchange value, generally is only the mode of expression, the phenomenal form, of something contained in it, yet distinguishable from it"<sup>91</sup>. If Positive economics, therefore, "sticks to appearances" only, then it expresses reality either one-sidedly or falsely. For example the examination of competition,

prices, supply and demand, etc., is a sufficient description of reality as it appears to the observer. Although this reality is a true one, it is, nonetheless, only a partial reality;

This positing of prices and their circulation etc. appears as the surface process, beneath which, however, in the depths, entirely different processes go on . . . It is the phenomenon of a process taking place behind it.<sup>92</sup>

Accordingly, if Positive economics takes the surface reality as the only reality then, evidently, it represents reality partially and insufficiently.

In addition, the phenomenal representation of reality poses the problem of a deceptive or a false representation of reality. For example, a deceptive account of reality occurs when Positive economics takes the exchange between capital and labour as an equal one: labour offers its services to capital in return for wages the level of which is determined by demand and supply. However, according to Marxism, labour offers more labour-time than is necessary for the production of goods sufficient for its survival and reproduction; the rest of the labour-time is surplus labour-time that is entirely appropriated by the capitalist. Thus the appearance of an equivalent exchange is deceptive because it hides an exchange of non-equivalents.<sup>93</sup>.

According to Marx,

This phenomenal form [wage-form], which makes the actual relation invisible, and indeed, shows the direct opposite of that relation, forms the basis of . . . all the mystifications of the capitalistic mode of production, of all its illusions as to liberty, of all the apologetic shifts of the vulgar economists.<sup>94</sup>

On the other hand, an epiphenomenal representation of reality may be false also due to the interpretation of appearances as natural and objective. This, according to Marxism, is an outcome of mistaking relations between people and classes for relations between things, that are assumed to be natural and everlasting. Hence the apologetic character of Positive economics, which considers, according to neoclassical principles, exchange and the market as the primary determinant of every economy, irrespective of time and place, and so justifies capitalism as the highest representative of this principle. This mystification occurs because relations between commodities appear as relations between things, whereas they are really relations between people and classes<sup>95</sup>. This type of mystification is called 'fetishism' and, according to Marx, it is

. . . peculiar to bourgeois Political Economy, the fetishism which metamorphoses the social, economic character impressed on things in the process of social production into a natural character stemming from the material nature of those things.<sup>96</sup>

However, this means that Positive economists are deluded not because they are ideologically biased - in a narrow sense - but because reality deludes them. The sense-perceptions and the observations of Positive economists are real, i.e. there is a one-to-one relationship between observations and phenomena, although they contain a part of reality that is in itself delusive, i.e. unequal things appear as equal, because they appear as material, are taken to be natural. "If then", according to Geras, "the social agents [and the Positive economist is a social agent] experience capitalist society as

something other than it really is, this is fundamentally because capitalist society presents itself as something other than it really is"<sup>97</sup>.

Consequently, according to the Marxist critique of empiricism, in its attempt to define reality in terms of observables, Positive economics commits the fallacies of defining it, (a) partially, and (b) falsely. Hence, if the critique is correct, any scientific procedure that validates theories in terms of a partial and false reality is bound to be partial and false in itself.

To summarise the major points made so far in this chapter: firstly, I emphasized the importance and relevance of the Problem of Induction and showed how its irresolution affects the rationality of Positive economics. Secondly, I indicated the general problems involved in the observation of the empirical world and hinted as to the difficulty of maintaining the analytic-synthetic distinction believed by Positive economists. Thirdly, I showed how the Problem of Induction affects the structure of Positive economics and how it renders the definition of empirical facts ambiguous. Fourthly, I examined whether the analytic-synthetic distinction holds in actual practice and found that the dichotomy breaks down once problems of identification and ceteris paribus conditions are brought into focus. Fifthly, I showed that logical and actual difficulties in the positivist analytic-synthetic distinction are related to the Problem of Induction. Lastly, I examined a critique of empiricism from the

point of view of an alternative methodology and concluded that empirical evidence represents reality falsely and insufficiently.

Having examined issues related to the nature of empirical evidence, and having found that empirical evidence is dependent on theory, that it is unjustified methodologically and that it represents reality partially and falsely, I now turn to investigate whether it represents reality reliably (i.e. accurately).

#### D. THE RELIABILITY OF EMPIRICAL EVIDENCE

Though the purpose of this chapter is to show the structure of economic facts from a definitional point of view, it is still worthwhile mentioning the extent to which economic facts are represented reliably. The sometimes extraneous or sometimes essential error creeping in economic data contributes to possible bias. However, in considering this bias in relation to theory selection it does not seem to be as important as the structural inconsistencies mentioned earlier. An ambiguous definition of what is to constitute economic evidence for the choice between rival theories seems to be more important in contributing to perpetual disagreement, rather than even a considerable error in the statistics. When an economic decision at the governmental level is based upon economic data, then inaccuracy of the statistics may make some difference to the decision taking. For instance, if foreign trade statistics show such an error as to produce a balance of payments deficit instead of a surplus for a particular month<sup>98</sup>, then government decisions will be wide of the mark. But as far as theory testing is concerned, more long-run evidence and repeated

observations are needed. In this case even if a considerable margin of error occurs, the results may play only an approximative role sufficient for the distinction between rival theories. A correlation of the money supply with income, for example, would span over a considerable period, and though it would incorporate errors<sup>99</sup>, this would not prevent Friedman from claiming the confirmation of his theory. The difficulty with this correlation is theoretical and implies a difficulty in the specification, thus impairing the exact stipulation of the direction of the lag of the correlation. As we shall see, most criticism against the money-income relation stems from theoretical, definitional and methodological difficulties rather than errors in the data<sup>100</sup>. Thus, in the case of theory testing, even if allowances are taken so as to render economic evidence relatively free from error, there are still conceptual, structural and inherent problems that render their interpretation ambiguous.

However, even though the inaccuracy of economic data may not seem as vital for the resolution of controversies, when compared with structural inconsistencies, it is worth noting that the unreliable representation of economic facts does contribute to the difficulties relating to the choice between rival economic theories. Errors of all kinds, from lack of precise economic concepts (and thus measurements) to hiding of information, render the task of theory choice cumbersome and the results unreliable<sup>101</sup>. As Morgenstern argues, "Whenever an economic argument is being made . . . the matter of the accuracy of . . . data arises"<sup>102</sup>. In what follows I shall briefly review some of the sources of error in economic data.

As we have seen in chapter 1 scientific procedure in Positive economics can be broken into four fundamental steps: data-model-computations-comparison with reality. To appreciate the role of error in relation to this procedure I shall quote a passage from Morgenstern in which he indicates that,

Each one of [the steps] has its own sources of error: the initial data are available only with a certain degree of accuracy (which it may be impossible to determine), the model is an idealization of reality, the computations can produce errors that are added to those existing at the start. The numerical result with all its cumulative errors will be compared with, and 'checked' against, a 'reality' that is again only revealed up to an (unknown) error factor. This is then hopefully called 'verification'.<sup>103</sup>

Economic models are supposed to be based on observations<sup>104</sup>. These observations are believed to represent reality. Given, however, the complexity of reality observations need to be designed. In physics data are collected through the direct observation of the scientist. In contrast, in economics observations are made according to data collected by people and organizations that most of the time have different purposes and ideas<sup>105</sup>. In consequence, the economist cannot, in principle, control the accuracy of the data. If, for any reason, there is distortion in the data, then the observations based on these data will carry on this distortion. Any discrepancy between reality and the collected data will lead to biased observations.

Granted that there is something which is called 'reality' and granted that empirical data represent it, can we, however, be certain that these data represent 'reality' reliably? Divergence

between data and reality may be caused by a variety of factors. For example data that deal with the economy as a whole are collected by governmental organizations, individual firms and various other agencies. As Friedman reports on his estimation of the money stock, "The basic data used . . . were not collected especially for this purpose but, like most statistical raw material, were largely an unintended by-product of governmental activities"<sup>106</sup>. In regard to governmental statistics, economists base their observations on data collected at different times and places<sup>107</sup> and for different objectives. The error inherent in these data may not affect the objective of the governmental organization, but it may affect the realism and the sophistication of the observations made by the economist. Significant distortion in these data may arise from various sources. For example, data on income, expenditure, savings, etc., are collected in an ad hoc manner without taking account of problems of conceptual classification<sup>108</sup>. Governmental agencies follow standards and conventions that do not necessarily reflect the finesse and abstraction of theoretical categories. More often than not these conventions are used for purposes of expediency with no proper regard given to definitional and classificational problems. According to Coddington, "Economic statistics are the result of the bureaucratic compounding of enormous quantities of fragmentary and even ambiguous pieces of information"<sup>109</sup>. Uncertainties and ambiguities of this kind may result in data revealing completely distorted situations. As Coddington reports differences of growth rates between what had actually happened and what was reported were found to be of the magnitude of five to six per cent<sup>110</sup>. However, more frequently

distortions are much smaller and more subtle, but they tend to become bigger once computations are carried on. The result is a cumulative increase of error. One cannot rely on the argument that errors will cancel out as the number of observations increases, unless there is evidence produced to confirm this<sup>111</sup>. The classical assumption of a normal distribution of errors, with zero mean and constant variance cannot hold in the case of misclassifications as "there are reasons to believe that the composite error may have a non-zero mean"<sup>112</sup>. Thus, operations on data compiled with wrong or ambiguous classifications will tend to magnify errors and bias will ultimately creep in.

Another indication of the inadequacy and inaccuracy of government statistics is that they are often revised, some almost month by month. Revision may take a long time and differences between preliminary and final estimates may be significant<sup>113</sup> (notwithstanding the fact that final estimates may also contain large error<sup>114</sup>). In a study made by Denton and Kuiper, it was found that bias in the estimation of parameters results from differences in revised data. According to their results "variations in parameter estimates are generally much greater between different sets of data than between methods of estimation"<sup>115</sup>. Furthermore, national account statistics are not compiled by following statistical sampling techniques but rather depend on answers from tax-returns, that may include false answers, or they are the by-products of administrative record-keeping, which may be conducted ambiguously. And yet the time

series, so often used by economists particularly in the Monetary controversy, depend upon these statistics<sup>116</sup>. Besides the error from misclassification that enters into the time-series data, there is another problem facing statistics compiled over time, in that decisions taken over a particular time interval may change, the quality of a product may differ, the degree of industrial concentration may vary, etc. The result of this instability in economic phenomena contributes to measurement problems and consequent error. Although improving techniques partially take account of this factor, nonetheless because of the uniqueness of economic phenomena, according to Morgenstern,

processes substantially changing in time are measured only once and cannot be measured more often. Thus the errors remain uncorrected and in extent unknown with each observation. No way of their elimination then exists . . .<sup>117</sup>

Problems also arise when statistics are compiled from private business organizations. There errors may result from possible falsification of information. For example, for reasons of competition, or from fear of tax-authorities, business firms tend to distort the report of data<sup>118</sup>. This is especially true in price statistics whereby firms tend to hide the true price of purchases and sales<sup>119</sup>. An additional difficulty with price statistics is the existence of non-price competition and the consequent measurement problems. Distortion might arise from monopoly practices, price discrimination, variations in quality and other non-price elements<sup>120</sup>. As far as wages are concerned, various fringe-benefits, or policy factors may also distort statistics on the price of labour<sup>121</sup>. Also statistics based on questionnaires may

be distorted due to false information, ambiguous stipulation of the questions or, as Morgenstern says, because of pessimism or optimism<sup>122</sup>, or due to the inclusion of concepts and ideas that produce different images from the ones expected by the investigator.

Finally, an additional source of error occurs through the summation of statistics taken from different phenomena when no qualitative differences are taken into account. Statistics derived from micro-variables are aggregated to give a statistic of a macro-variable. However, in this process bias arises due to possible functional inter-relations between the variables. The assumption of independence between economic units, although it may be valid for purposes of abstraction, cannot hold true for the complex and continuous reality, whereby "macro-variables are functionally related to the micro-variables, the micro-variables to each other, and the macro-variables to each other"<sup>123</sup>. Also aggregation or sectoring may be based on criteria that change from country to country, or from period to period<sup>124</sup>. The consequence is that statistics based on aggregate movements may include bias (which according to Morgenstern becomes more inclusive through summation<sup>125</sup>) that may distort 'true' (surface) reality.

In reviewing various sources of error in economic data I did not intend to cover all possible factors of distortion. Obviously other major causes of bias exist and are as important. For instance, definitional problems are significant since they may yield different sets of data for the same period or place<sup>126</sup>, or problems of quantifying hazy economic concepts<sup>127</sup>, prohibit the undistorted correspondence

between numerical data and 'reality'<sup>128</sup>. However, sufficient for the purposes of this chapter was to indicate that empirical testing in economics has to rely on data that do not enjoy the precision, adequacy and reliability of the data in experimental sciences.

Although the unreliability of empirical evidence is a relatively significant factor in contributing to the difficulties facing the validation of economic theories, nonetheless these difficulties are greatly enhanced when one takes into consideration the structural inconsistencies embodied in the methodology of Positive economics. As noted in the beginning of this section, in a given economic controversy, as will be seen in the following chapters, structural reasons related to theory and methodology seem to play a more significant role in the persistence of the controversy, rather than the inadequacy of empirical data. The reason is that in the case of theoretical conflict the inadequacy of data faces both members of the conflict equivalently. Either member has to deal with equally unreliable observations. Excluding forgery of data, two rival economic claims confront equally inadequate evidence. Although inadequacy of data may lead to unreliability of the theory being supported by the data, both theories are potentially equally unreliable, given the data. The choice then has to be made according to these inadequate, unreliable and approximate data. Thus, although unreliable data weaken the positivist position of empirical testing, nonetheless inherent difficulties with this methodology stem mainly from problems and contradictions within the procedure itself.

## E. CONCLUSION

In this chapter I have attempted to describe certain logical and actual inconsistencies that arise from within the structure of Positive economics. In examining the nature of empirical evidence I have indicated that the problem of justifying induction produces a logical impasse. Where induction forms the corner-stone of scientific validation, this impasse creates problems that need to be solved. In the case of Positive economics the distinctions made in order to overcome these problems are, however, inapplicable. They constitute only ambiguous mediations between the logical impasse and the actual practice of acting as a Positive economist (i.e. testing theories empirically). Furthermore, the ambiguous character of these distinctions infiltrates the definition given to empirical evidence by Positive economists, with the consequence of persistent polar views, such as in the case of the Monetary controversy.

In addition I have examined the definition of reality within Positive economics from the point of view of an alternative methodology and have found that reality in Positive economics, being a surface reality, is partial and deceptive. Finally, I have indicated that apart from structural incongruities in the nature of empirical evidence, and even within the domain of 'surface reality', there are also actual inadequacies characterising the evidence itself. Not only, therefore, does empirical evidence represent reality ambiguously and deceptively but also unreliably.

Having, therefore, examined the structure of the methodology of Positive economics, I shall now turn to examine how this methodology works in practice. In what follows I shall describe the Monetary controversy and see whether empirical evidence and testing have, in fact, been able to resolve the conflict. This examination will constitute Part II of the thesis. In Part III I shall critically review some of the criticism levelled against Positive economics, and then I shall put forward a full explanation of the persistence of disagreement in Positive economics.

## FOOTNOTES

## PART I: CHAPTER 2:

1. A Treatise of Human Nature, Book 1, Part IV, Section III, p.223.
2. For example, Morgenstern in trying to expose the relationship between economic theory and experience reflects this ambiguity when he claims that, "The two approaches are necessarily inseparable from each other; but they need to be further characterized in their natural relation to each other", "On the Accuracy of Economic Observations", op.cit., p.93. See also, Schumpeter, "The Common Sense of Econometrics", op.cit., p.12. Or, as Koopmans asks, "How much of it (economic knowledge) is derived from observation, how much from reasoning?" "The Construction of Economic Knowledge", in his "3 Essays on the State of Economic Science", op.cit., p.131.
3. B. Russell, The Problems of Philosophy, 1912, p.35.
4. See A. Lowe, "Toward a Science of Political Economics", in Heilbroner's "Economic Means and Social Ends", op.cit., pp.2-3.
5. See Hollis and Nell, "Rational Economic Man", op.cit., pp.12,27,81, and p.83.
6. Culbertson, "Macroeconomic Theory . . .", op.cit., pp.113-114, "if (our activities) cannot be justified, the economists become an interest group", ibid., p.115.
7. Hollis and Nell, op.cit., p.28.
8. Russel, op.cit., p.38. Although the Problem of Induction is quite obvious to philosophy students, I will discuss it in some length, firstly, because its ambiguity is related to the ambiguities discussed above, secondly, because it sheds some light on the question of independence between economic theory and observation, and thirdly, because it is vital in the understanding of economic controversies.
9. Hollis and Nell, "Rational Economic Man", op.cit., p.11. The Problem of Induction has been stated by philosophers as follows: starting appropriately with Hume, he says that "even after the observation of frequent or constant conjunction of objects, we have no reason to draw any inferences concerning any object beyond those of which we have had experience", "A Treatise on Human Understanding", op.cit., Book I, Part III, Section XII, p.139. Russell also says that "When two things have been found to be often associated, and no instance is known of the one occurring without the other, does the occurrence of one of the two, in a fresh instance, give any good ground for expecting the other?", ibid., p.36. Also as Barker puts it, "what right have we to suppose that certain information about what has been observed can confirm certain hypotheses about what has not been observed?", Induction and Hypothesis: A Study of the Logic of Confirmation, 1957, p.10, cf. also Lazerowitz, "Philosophy and Illusion", op.cit., p.228.

## FOOTNOTES (cont.)

## PART I: CHAPTER 2:

10. See Russell, "The Problems of Philosophy", op.cit., p.34.
11. Quoted in Lazerowitz, "Philosophy and Illusion", op.cit., p.229. See also Russell, ibid., pp.40-41. In addition if the Logical Positivist position is taken, of Ayer for example, then we would have to describe logical propositions as tautologous and therefore not having any true or false value. In consequence they could not be used in order to validate the Principle of Induction. See A.J. Ayer, Language, Truth and Logic, 1946, p.50. Katz also adds that, "inductive arguments cannot be justified on deductive grounds", The Problem of Induction and its Solution, 1962, p.5.
12. Economic Philosophy, 1962, p.7, or as Katz comments, "If there is no better justification for one method than for another, there is no better reason for one belief than for another", ibid., p.18.
13. See Russell, "The Problems of Philosophy", op.cit., p.35.
14. See Ward, "What's Wrong with Economics", op.cit., p.161.
15. See Morgenstern, "On the Accuracy . . .", op.cit., pp.48-50.
16. Russell, ibid., p.35.
17. Barker, "Induction and Hypothesis", op.cit., pp.50-55. Cf. Russell, ibid., p.36.
18. J.M. Keynes, "A Treatise on Probability", The Collected Writings of J.M. Keynes, Vol.VIII, 1973.
19. Quoted in R.B. Braithwaite, "Probability and Induction", in C.A. Mace, British Philosophy in the Mid-Century: A Cambridge Symposium, 1966, p.147.
20. Ibid., p.146. See also K. Popper, The Logic of Scientific Discovery, 1968, pp.29-30.
21. See Hollis and Nell, "Rational Economic Man", op.cit., pp.75-78.
22. Barker, ibid., p.32.
23. Braithwaite, in the Introduction to the Treatise, p.XXII.
24. See Keynes, ibid., pp.6,246. Ramsey criticized Keynes' solution by claiming that "the suggestion of Mr. Keynes that (the justification of inductive inference) can be got around by regarding induction as a form of probable inference cannot in my view be maintained". F.P. Ramsey, The Foundations of Mathematics and Other Logical Essays, 1931, p.197.

## FOOTNOTES (cont.)

## PART I: CHAPTER 2:

25. See Hollis and Nell, ibid., p.77. See also F.C. Mills, "On Measurement in Economics", in R.G. Tugwell "The Trend of Economics", op.cit., pp.58-61, and Cohen, "Reason and Nature", op.cit., p.355.
26. Hollis and Nell, ibid., p.76.
27. "Probability and Confirmation . . . are derivative concepts, usable only after the riddle of Induction has been solved", Hollis and Nell, ibid., p.75, also pp.76-77, 11.
28. Keynes, op.cit., p.468.
29. A review of solutions to the Problem of Induction is given in Lazerowitz ("Philosophy and Illusion", op.cit., pp.218-257), Barker ("Induction and Hypothesis", op.cit., pp.1-23) and R. Bhaskar, A Realist Theory of Science, 1975, pp.215-228. It seems to me that putting the difficulty of justifying induction in terms of 'problem-solution' involves a logical inconsistency. To begin with, for a problem to merit a solution it first of all has to be a problem. Besides Ayer who dismissed the Problem of Induction by stipulating a clear distinction between tautologous analytic statements and synthetic meaningful statements, and relegating the Problem to analytic and thus meaningless statements (Language, Truth and Logic, 1946, pp.49-50), most philosophers have taken the position that the problem exists and requires solution. However, in what follows I would like to show that not only is the Problem of Induction ambiguous but also logically, and in its own terms, absurd. Once the Problem of Induction is stated, and once the Problem of Deduction prevents the use of deduction for its solution, and therefore both empirical and a priori criteria are eliminated from its justification - and thus knowledge of the existence of things is doubted - then logically we can have neither empirical nor theoretical criteria, nor any other criteria whatsoever, to justify our awareness or knowledge of the problem itself. In other words, in itself once we state the Problem of Induction and accept its conditions, and thus its irresolution, then we must necessarily accept its non-existence, which is absurd. The self-contradictory nature of the problem, however, does not imply that the difficulty does not exist. Once induction is used we have to justify its use. Since we cannot bring forward a justification then either we have to use induction and forego any logical justification or stop using induction as a valid means of inference. Induction becomes self-contradictory once we view it as a problem that requires a rational solution.
30. Lazerowitz, ibid., p.232. see also Ward, "What's Wrong . . .", op.cit., pp.161-162, and Katz, who says that "many of the most influential philosophers in modern philosophy . . . have grappled with the problem. But the problem has remained as much a riddle as Hume left it", "The Problem of Induction . . .", op.cit., p.X, and p.6.

## FOOTNOTES (cont.)

## PART I: CHAPTER 2:

31. For the use of the term 'mediation' in relation to contradiction see, E. Leach, "Genesis as Myth", op.cit., pp.10-11.
32. Carnap quoted in M. Spector's "Theory and Observations", British Journal for the Philosophy of Science, 1966, p.1. For the positivist syndrome see L. Kolakowski, Positivist Philosophy, 1972, esp. pp.205-6, and Ayer, "Logical Positivism", Introduction, op.cit., pp.3-28.
33. Russell, "Problems of Philosophy", op.cit., pp.3-4. Furthermore, since the object itself remains unknown and its 'essential properties' are hidden, observation becomes, according to Positivists, possible only through external behaviour. According to Braithwaite, "Observed facts are usually facts about the behaviour of material bodies", (quoted in Spector, ibid., p.4).
34. Russell, ibid., pp.25-6.
35. Ibid., p.25. But even at the level of sensations Hanson, for instance, doubts the idea of theory independent sense perceptions; "Theories and interpretations are 'there' in the seeing from the outset", Patterns of Discovery: An Inquiry into the Conceptual Foundations of Science, 1958, p.10.
36. G. Ryle, The Concept of Mind, 1949, pp.194,212.
37. Ibid., p.194. Morgenstern also comments that "there is a fundamental difference . . . between mere data and observations. The latter are naturally also data, but they are more than that. They are selected". "On the Accuracy . . .", op.cit., p.88. However, in economics even the distinction between sensations and observations cannot be held, since we cannot have sensations of inflation or unemployment, but only observations.
38. Russell, "Problems of Philosophy", op.cit., p.25.
39. "Observation of x is shaped by prior knowledge of x", Hanson, ibid., p.19.
40. See also T.S. Kuhn, "Logic of Discovery or Psychology of Research" in Lakatos and Musgrave, "Criticism and the Growth of Knowledge", op.cit., p.2. Also "The Structure of Scientific Revolutions", op.cit., pp.52,66.
41. Spector, "Theory and Observation", op.cit., pp.5-7.
42. For instance Maxwell offers a criterion in terms of how quickly one decides to identify the property of one thing or another. He says, "'observation term' may now be defined as a 'descriptive . . . term which may occur in a quickly decidable sentence'", quoted in Spector, "Theory and Observation", op.cit., p.6.

## FOOTNOTES (cont.)

## PART I: CHAPTER 2:

43. P. Feyerabend, "Problems of Empiricism", in R. Colodny's Beyond the Edge of Certainty, 1965, pp.151-152. Hanson also adds that, "which are the data-words and which the theory-words is a contextual question", "Patterns of Discovery", op.cit., p.59, see also pp.19-21.
44. F.A. Hayek, "Scientism and the Study of Society", Economica, 1943, pp.35-7.
45. Barker, "Induction and Hypothesis", op.cit., p.39. The issue about the interdependence between theory and observation has taken quite a prominent place in the discussions on the philosophy of science. See for instance, M. Hesse, "Is There an Independent Observation Language?", in R. Colodny's The Nature and Function of Scientific Theories, 1970, p.35.
46. See E. Nagel, "Theory and Observation", in E. Nagel, S. Bromberger, A. Grunbaum, Observation and Theory in Science, 1971, pp.17-26.
47. For a critical review of the controversy see Lazerowitz, "Philosophy and Illusion", op.cit., pp.119-40.
48. Alongside logical reasons there are also actual reasons for this breakdown, such as Heisenberg's principle of uncertainty or Maxwell's principle of complementarity; see R.C. Linstromberg, "The Philosophy of Science and Alternative Approaches to Economic Thought", Journal of Economic Issues, 1969, pp.188-9, and K. Boulding, "Economics as a Moral Science", American Economic Review, 1969, p.3.
49. See Hollis and Nell, "Rational Economic Man", op.cit., p.201.
50. Friedman, "The Methodology . . .", op.cit., pp.15,30. "For us the test is strictly pragmatic", M. Friedman and A.J. Schwartz, Monetary Statistics of the U.S.: Estimates, Sources, Methods, 1970, p.1.
51. This argument, however, is accepted by Friedman. His position centers around the concept of disconfirmation, which will be criticized below.
52. See Hollis and Nell, ibid., p.79, Russel, "Problems of Philosophy", op.cit., pp.35-6.
53. An example of this is Emmer who says that, "Induction must be more than a habit; it must be an instinct, a way of operating built into the very structure of the nervous system", R. Emmer, Economic Analysis and Scientific Philosophy, 1967, pp.28-29.
54. Lipsey, "Introduction . . .", op.cit., p.11.

## FOOTNOTES (cont.)

## PART I: CHAPTER 2:

55. See Hollis and Neill, ibid., pp.-98-201.
56. Ibid., pp.198-201.
57. See Barker, "Induction and Hypothesis", op.cit., esp. pp.62-90.
58. See J. Hicks, "What is Wrong with Monetarism", Lloyds Bank Review, 1975, p.3.
59. Friedman, "The Methodology . . .", op.cit., p.9. "The evidence for a hypothesis always consists of its repeated failure to be contradicted", ibid., p.23. A more sophisticated Popperian falsificationism for economics is taken by J. Agassi, "Testability and Tautology in Economics", Philosophy of the Social Sciences, 1971, pp.49-64.
60. Lipsey, "Introduction . . .", op.cit., p.5.
61. See Bhaskar, "A Realist . . .", op.cit., p.218, and Ward, "What's Wrong . . .", op.cit., p.166.
62. For a critique of Popperian empiricism see K. Williams, "Facing Reality - A Critique of Popper's Empiricism", Economy and Society, 1975, pp.309-358. Williams attacks falsificationism in terms of the conditions Popper attaches in controlling the theorist. For a different criticism see Bhaskar, ibid., pp.217-8.
63. Coddington, "Positive Economics", op.cit., p.10.
64. Ibid., p.8.
65. Friedman, "The Methodology . . .", op.cit., p.7.
66. Ibid., p.34. For the same observation cf. Coddington, "Positive Economics", op.cit., p.10, and Hollis and Neill, "Rational Economic Man", op.cit., p.200. A curious observation is that Friedman, in page 34, uses quotation marks when referring to facts, whereas before he did not feel the need to do so.
67. Samuelson, "Economic Theory and Mathematics", op.cit., p.57.
68. Samuelson, "Economics", op.cit., p.9. For similar inconsistencies see also Sir Henry Clay, "Facts and Theory in Economics", in P. Samuelson's "Readings in Economics", 1952, esp. pp.3-4. In a different context Samuelson reports of "genuine methodological puzzles" such as, for example, that "what is a fact . . . is a very subjective thing", "Theory and Realism: Reply", op.cit., pp.738-739.

## FOOTNOTES (cont.)

## PART I: CHAPTER 2:

69. Friedman would argue, however, that the independence is achieved once predictions are confirmed (or failed to be disconfirmed). Although we may need theories to order and interpret the complex empirical world, these theories are or are not justified *a posteriori*. In other words the independence criterion is fulfilled once the theory is rendered valid by its predictive success. In effect only the importance of a *posteriori* criteria are emphasized with no active role for theory. Thus theories are useful "filing systems" and they are justified once their predictions are systematically confirmed. However, Friedman begs the dependence question, since he cannot have criteria of independence between the theory-fact mixture that stands as evidence for the confirmation of predictions. Predictions are tested by facts, and facts are assumed, *ab initio*, to be dependent on theory.
70. Coddington, for instance, says that, "the recent controversies in monetary theory . . . and in capital theory . . . have not been brought any nearer to resolution by the introduction of evidence by either side, since part of the controversy in each case concerns the status and interpretation of such evidence;"; "Positive Economics", *op.cit.*, p.9.
71. See chapter 5, pp.188-191.
72. *Ibid.*, pp.183-184.
73. Friedman, "A Theoretical Framework . . .", *op.cit.*, p.1.
74. The relationship between 'analytic-synthetic', deductive-inductive and methodological controversy is depicted very well in P. Mini's review of Hollis and Nell's, "Rational Economic Man", *History of Political Economy*", 1977, p.291.
75. Culbertson, in fact, implies this ambiguity when he remarks that "The difference in view of macroeconomic theory about 'what causes what' can be interpreted as arising from imprecision in statements of the domain of applicability of empirical propositions, or of the limits of valid generalization of parameter values estimated from sample data", "Macroeconomic Theory . . .", *op.cit.*, p.109.
76. Hollis and Nell, *ibid.*, p.74.
77. As Grahm points out, "In the context of applied macroeconomics, that is to say, the movements of the economic system exemplify in an acute manner a central dilemma in all social sciences: . . . how do we disentangle the determinant of a social process from its concomitants, cause from concurrence . . . this fundamental lack of information permits the survival of the most diverse and contradictory theoretical positions . . ." "Econometrics and Economics", *op.cit.*, p.12. See also Culbertson, "Macroeconomic Theory . . .", *op.cit.*, p.97.

## FOOTNOTES (cont.)

## PART I: CHAPTER 2:

78. L.R. Klein, An Introduction to Econometrics, 1962, p.14. Also Wonnacot and Wonnacot specify the identification restrictions in terms of a priori information, see their Econometrics, 1970, pp.175-180.
79. Klein, ibid., p.67. Also Hollis and Nell argue that, "identification is crucial and depends on prior theory", "Rational Economic Man", op.cit., p.83.
80. See Klein, ibid., p.18, and Hollis and Nell, ibid., p.27.
81. See L. Kolakowski, "Positivist Philosophy", op.cit., pp.9-11.
82. Ibid., p.15.
83. See, e.g., S. Schoeffler, The Failures of Economics: A Diagnostic Study, 1955, Ward, "What's Wrong . . .", op.cit., esp. pp.240-3.
84. And thus as a distinct philosophical school, "Positivism stands for a certain philosophical attitude to human knowledge . . . [I]t is a collection of rules and evaluative criteria . . . [it] is a normative attitude . . .", Kolakowski, ibid., pp.10-11.
85. E. Rosdolsky, "Comments on the Method of Marx's Capital and Its Importance for Contemporary Marxist Scholarship", New German Critique, 1974, p.66.
86. M. Godelier, Rationality and Irrationality in Economics, 1968, p.XIX.
87. K. Marx, Grundrisse, 1973, p.101.
88. M. Nicolaus, "Forward" to the "Grundrisse", op.cit., p.27. Also Godelier adds that "a structure is not a reality that is directly visible, and so directly observable, but a level of reality that exists beyond the visible relations between men, and the functioning of which constitutes the underlying logic of the system, the subjacent order by which the apparent order is to be explained", ibid., p.XIX.
89. M. Godelier, "System, Structure and Contradiction in Capital", Socialist Register, 1967, p.93.
90. K. Marx, Capital, Vol.I, 1974, p.291, see also p.504 and p.533 or where he claims that "Political economy sees only what is apparent . . .", in Capital, Vol.II, p.128.
91. Marx, Capital, Vol.I, op.cit., p.45.

## FOOTNOTES (cont.)

## PART 1: CHAPTER 2:

92. Marx, "Grundrisse", op.cit., p.247 and p.255, and where he says that "the relation of exchange . . . is a mere semblance", ibid., p.458.
93. According to Nicolaus, "The exchange of equivalents is the fundamental social relation of production, yet the extraction of non-equivalents is the fundamental force of production", M. Nicolaus, "The Unknown Marx", in P. Blackburn's Ideology in Social Science, 1972, p.325. Another example of deceptive appearance is the empirical measurement of profit, which involves the difference between costs and revenues, and so conceals the exploitative nature of the transaction between capital and labour. Profits in order to be fully accounted for must be derived from surplus value. As Nicolaus says, "The rate of profit . . . actually falsifies the rate of exploitation, and falsifies it to a higher degree as capitalism develops", Nicolaus, "Forward", op.cit., p.26.
94. Marx, "Capital", Vol.I, pp.505-506, see also p.577. For a discussion of the reflective, deceptive and false function of surface reality and the wage-form see N. Geras, "Marx and the Critique of Political Economy", in Blackburn's "Ideology . . .", op.cit., pp.248-305, R. Bhaskar, "Marx's Concept of Ideology", unpublished paper presented in a seminar in the Department of Philosophy at the University of Edinburgh, and R. Gunn, "Marx's Methodology" unpublished paper presented in a seminar in the Department of Politics at the University of Edinburgh.
95. See F. Engels, "K. Marx, A Contribution to the Critique of Political Economy", in Selected Works, 1973, p.514.
96. Marx, "Capital", Vol.II, p.229. Also Geras says that, "The mechanism of mystification consists in the collapsing of social facts into natural ones", "Marx and his Critique . . .", op.cit., p.295.
97. Geras, ibid., p.296.
98. See, e.g., A. Coddington, "Are Statistics Vital?", The Listener, December 1969, p.822.
99. In fact Friedman himself acknowledges the unreliability of data when he says that there are ". . . common problems in constructing acceptable estimates of aggregate monetary totals from the banking statistics" which stem from "incompleteness of coverage, ambiguity of reported data, inaccuracy of reported data, and divergent dating of the data", "Monetary Statistics of the U.S.", op.cit., p.211.

## FOOTNOTES (cont.)

## PART I: CHAPTER 2:

100. Although some reviewers of the Monetary debate would claim that "Gaps in the available data . . . make the testable structure a shadowy replica of its theoretical counterpart and distort the empirical results", they would not put the burden of the irresolution of the debate only on this factor. See G.R. Fisher and D.K. Sheppard, "Interrelationships between Real and Monetary Variables: Some Evidence from Recent U.S. Studies", in H.G. Johnson and A.R. Nobay, Issues in Monetary Economics, 1974, pp.180,182.
101. For example the establishment of constant ratios is a source of dispute due to errors and unreliability of data. According to Morgenstern, "Economic constants, whether Pareto's  $a = 1.5$ , the capital/output ratio, or the ratio of income between labor and capital, etc., all are based on observations and measurements. Hence they have to be reconciled with the errors in the data and since these are likely to be considerable, these different ratios and constants lose much of their definiteness, which accounts for the fact that there is much controversy in the area", "On the Accuracy . . .", op.cit., p.50, see also p.52.
102. Morgenstern, ibid., p.6.
103. Morgenstern, op.cit., pp.94-95.
104. Cf. F.C. Mills, "On Measurement in Economics", in Tugwell, The Trend of Economics, op.cit., pp.37-70, where he contrasts older types of economic speculation to modern theory that is dependent on observations.
105. See, S. Kuznets, Quantitative Economic Research: Trends and Problems, 1972, pp.7-9, and Morgenstern, ibid., pp.12-14.
106. "Monetary Statistics . . .", op.cit., p.2.
107. See Kuznets, ibid., p.9.
108. See M. Copeland, Statistics and Objectivity in Economics: The Testament of an Institutionalist, 1958, pp.72-3, and Coddington, "Are Statistics Vital?", op.cit., p.823.
109. Coddington, ibid., p.823. Cf. also Kusnets, ibid., p.18. Also Morgenstern adds that, "if definitions are uncertain, very frequently, and classifications are subject to much doubt, the data must reflect these conditions even though the convenient normal notion of 'error' is not directly applicable", ibid., pp.38-39. See also L. Klein, "Empirical Evidence on Fiscal and Monetary Models", in S.J. Diamond, Ed., Issues in Fiscal and Monetary Policy, 1971, p.41.

## FOOTNOTES (cont.)

## PART I: CHAPTER 2:

110. Coddington, ibid., p.823. Another example is given in the report of the Price Commission where it was found that, "The sums which lay behind one of the most important relaxations of price controls . . . have turned out to be subject to an error of more than 50 per cent", J. Carvel, "Government's Sums Turns Out to be Wrong", Guardian, 13/5/1975.
111. Morgenstern, for instance, claims that, "any statement that errors 'cancel', neutralize each other's influence, has to be proved. Such proofs are difficult and whether a 'proof' is acceptable or not is not easy to decide", ibid., p.53.
112. T.W. Murray, "An Empirical Examination of the Classical Assumptions Concerning Errors in Data", Journal of the American Statistical Association, 1972, p.532. See also Coddington who says that, "in the absence of any evidence, one must conclude that errors are as likely to cumulate as they are to compensate", ibid., p.823.
113. See Coddington, ibid., p.823.
114. See T.F. Denton and J. Kuiper, "The Effect of Measurement Errors and Parameter Estimates and Forecasts: A Case Study based on the Canadian Preliminary National Accounts", Review of Economics and Statistics, 1965, p.199.
115. Ibid., p.201.
116. Ibid., p.198. For the difficulties in the evidence used in the Monetary controversy see Fisher and Sheppard, "Interrelationships between Real and Monetary Variables: Some Evidence from Recent U.S. Empirical Studies", in Johnson and Nobay, "Issues in Monetary Economics", op.cit., esp. p.180, and A. Ando and F. Modigliani, "The Relative Stability of Monetary Velocity and the Investment Multiplier", American Economic Review, 1965, p.714.
117. Morgenstern, op.cit., p.46, and also pp.43-50.
118. See Morgenstern, ibid., p.17.
119. Ibid., p.182.
120. See R. Cox, "Non-Price Competition and the Measurement of Prices", Journal of Marketing, 1946, p.371, and Morgenstern, op.cit., p.181 and 184.
121. Morgenstern, ibid., p.186.
122. Ibid., p.56.

## FOOTNOTES (cont.)

## PART 1: CHAPTER 2:

123. K. Lancaster, "Economic Aggregation and Additivity", in Krupp's "The Structure of Economic Science", op.cit., p.202.
124. See Kuznets, "Quantitative Economic Research", op.cit., pp.14-5.
125. Morgenstern, ibid., p.51.
126. See Kuznets, ibid., p.18, and G. Studdart-Kennedy, Evidence and Explanation in Social Science, 1975, p.14.
127. Coddington, "Positive Economics", op.cit., p.11.
128. Coddington, "Soft Numbers and Hard Facts", op.cit., p.579.

P A R T    II

T H E    M O N E T A R Y    C O N T R O V E R S Y

A N D    P O S I T I V E    E C O N O M I C S

"What generates in me a great deal of scepticism about the state of our discipline is the high positive correlation between the policy views of a researcher . . . and his empirical findings. I will begin to believe in economics as a science when out of Yale there comes an empirical Ph.D. thesis demonstrating the supremacy of monetary policy in some historical episode - and out of Chicago, demonstrating the supremacy of fiscal policy."

Don Patinkin<sup>1</sup>

## P A R T II

THE MONETARY CONTROVERSY  
AND POSITIVE ECONOMICS

## OUTLINE

The purpose of this part is to provide a testing ground of the Positive economic claim that empirical facts ought to discriminate between valid and non-valid theories. Having seen in the previous part that Positive economic methodology finds difficulties in fulfilling logical and actual criteria for justifying this claim, I shall now examine whether, in fact, it works in practice. If it is shown that the controversy persists despite the accumulation of empirical evidence, then one may argue that neither in theory nor in practice does theoretical conflict find resolution in appealing to empirical evidence.

I have chosen the Monetary controversy as a case study for the following reasons: in the first place it is a major controversy, and an example par excellence of the application of empirical testing to rival economic claims. In other words it is an ideal case for seeing Positive economics in action. In the second place there are two distinctively opposed groups each one embracing a particular hypothesis that purports to account for the same phenomenon, namely the determination of macroeconomic aggregates and the delineation of the relative impact that various policy instruments have upon these aggregates. Moreover, both sides offer a great deal of empirical evidence that is supposed to furnish sufficient proof of the truth of either

hypothesis. In the controversy one can also find a multitude of types of evidence, starting from single-equation tests and structural models, to qualitative judgements. In the third place both groups belong to the paradigm of Positive economics. They are supposed to apply the same principles of validation assigned by the methodology of Positive economics. However, their interpretation of this methodology is different. As has been shown in chapter 1 ambiguities in the structure of Positive economics produce conflict as to the appropriate interpretation of theory and fact, viz., Predictionists versus Assumptionists. Although the resulting F-Twist controversy manifests itself also in the methodological rivalry characteristic of the Monetary controversy, nonetheless the broad purpose of both Monetarists and Fiscalists is to test their theoretical claims with empirical facts.

The aim of Positive economists is to construct theories out of observation and test them with empirical facts. One test of Positive economics, therefore, is the extent to which its supporters follow its rules and do not diverge from them, as this would imply that either the rules are not functioning properly, and therefore need adjustment, or that economists adhere to their own private methodology while disregarding the rules. In this latter case Positive economics would have only nominal value without any real consequence. The point about the Monetary controversy is that although in theory economists are willing to follow strictly the rules stipulated by Positive economics (and they think they do<sup>2</sup>), in practice they adjust them in order to suit their purposes. As we shall see the Monetary controversy is a testimony against the pure methodological claims of Positive

economics. Empirical evidence is mixed with value-judgements and testing procedures are impregnated with rival definitions and theoretical specifications. There has been almost no case in which one group accepted the evidence of the other with no methodological or theoretical qualifications. It seems, therefore, that Positive economics finds difficulties in offering an unequivocal and objective methodology able to resolve theoretical conflict. As we shall see in Part III these practical difficulties are manifestations of the logical difficulties discussed in Part I, which combine and are structured in such a way so as to render any attempt to resolve the Monetary controversy, within the walls of the paradigm of Positive economics, virtually impossible.

In studying this controversy I shall briefly sketch out its post-war development in relation to the fluctuations of economic activity and policy, so as to acquire a view of the link and dialectic effect between the success and failure of a policy measure and the stimulation or justification it gives to the theory reflecting it. Then I shall indicate some of the differences and particular characteristics of each hypothesis and possibly clarify some misunderstandings involved in the conception one group has for the theoretical framework of the other. The analysis will be concentrated only on those differences that seem to be consequential on the outcome of empirical research. In addition, I shall try to differentiate between theoretical and empirical issues and narrow down as far as is possible the grounds upon which the battle is fought. The scope of the analysis, however, will be quite wide referring generally to the major differences, disregarding

the less important ones and leaving aside the intensive analysis of each particular issue. Finally I shall examine the kinds of evidence advanced by both sides and try to see whether there exists a set of clearly defined and presented evidence that is accepted by both sides and which can be definitely called an economic fact, be commonly accepted, and be able to become the arbiter between the opposing hypotheses.

Having made such an examination I hope to bring out the idea that the Monetary controversy provides an appropriate experiment, the results from which can be used to test the methodological hypothesis of Positive economics that empirical evidence furnishes absolute criteria of theory validation.

CHAPTER 3

ECONOMIC POLICY  
AND THE DEVELOPMENT  
OF THE CONTROVERSY

## CHAPTER 3:

ECONOMIC POLICY AND THE DEVELOPMENT  
OF THE CONTROVERSY

## A. POST-WAR MACROECONOMIC POLICY

Although the Monetary controversy dates back to Ricardo and the Bullionists and passes through the Currency and Banking schools conflict in the 19th century and through the Economic Depression, its growth takes enormous proportions during the period after the Second World War. This period, namely from 1950 to 1970, offers two types of evidence one of which was not available to the older disputes. The first one concerns the outcome of various policies, the success or failure of which were taken to justify the truth or falsity of the theories behind the policies - common to old and new discussions - and the second one concerns the accumulation of 'hard' empirical evidence, i.e. data produced through econometric analysis. This latter development, prima facie at least, seems to have been a decisive turning point in the scientific effort to find criteria of choice between the alternative hypotheses. However, it became eventually apparent that the power of this new type of evidence was not proved to be as decisive as it was thought it would be. The controversy, despite the accumulation of empirical evidence from both sides, still remains unresolved.

For the moment I shall leave this latter type of evidence aside until chapter 5 and first give a rough account of the development of monetary and fiscal policy in the period from 1950 to 1970, as this will set out the historical perspective and context through which to

look at the controversy. The choice of this period is significant because it contains the most important turning points that account mainly for the apparent failure of Keynesian policy and for the consequent 'counter revolution' of Monetarism<sup>3</sup> partly brought about by the success of monetary policy<sup>4</sup>. The increase in the intensity of the controversy is tied up to the rise of Monetarism as a separate school, according to its new version given by the Chicago school and Friedman. Monetarism became more apparent with the economic events around the middle and late '60s especially in the United States. I have put the emphasis on events in the American economic scene because, firstly, it is the country where Monetarism grew and intensified the controversy, and secondly, because it is the country where the greatest part of the debate literature is being written.

In accounting for the chronological development of the controversy I separate the post-war period into two sub-periods, firstly from 1950 to 1960, and secondly from 1960 to 1970.

a. 1950-1960

This particular sub-period is considered to be less important than the one following it; yet it is significant because it saw the emergence of an independent Federal Reserve System in the United States, reflecting the need for an official implementation of Keynesian policy, and because it included the peak of the rise of Keynesian<sup>5</sup> macroeconomic thought and policy, the "high-tide" of Keynesianism as Johnson puts it<sup>6</sup>. This peak has its causes in the apparent<sup>7</sup> success Fiscalism enjoyed during the interwar period. Before the Great Crash Monetarism was

considered a very potent and effective theory, and ". . . with respect to the early 1930's, it can be said that the United States in the past has relied in large part on monetary policy as its major instrument for achieving price stability and high employment"<sup>8</sup>.

However, the climate of the aftermath of the Depression, i.e. the interpretation of the ineffectiveness of monetary theory due to very low levels of interest rate, and the publication of Keynes' "General Theory" in 1936, brought about the collapse of the Quantity Theory and the emergence of an era of macroeconomic thinking based upon the Keynesian explanation of economic reality. This was accentuated by the fact that national economies immediately after the Second World War enjoyed a relatively high economic growth believed to be attributed to the application of Keynesian theory<sup>9</sup>. However, underlying inflationary tendencies, and some 'voices' crying in the 'wilderness of Keynesianism' (mainly Friedman's<sup>10</sup>) becoming eventually heard, started to discredit the Keynesian thesis.

The mark of this period was the publication of the Radcliffe Report in 1959, which reflected both the peak and the trough of Keynesianism. The peak because it offered an extreme - and unsophisticated - Keynesian policy disregarding the control of changes in the money supply, and the trough because of the immense criticism that it received for its extremity. In fact, due to the overdue neglect of monetary policy, its proposals - at least for the academic economists - were relatively neglected. It was alleged that its propositions were contradicted by the empirical facts<sup>11</sup>.

In effect the Radcliffe Report heralded the beginning of the downfall of Fiscalism and the intensification of the controversy. Although the Radcliffe Committee acknowledged the relative importance of Monetary policy<sup>12</sup> it nevertheless, by following a strong Keynesian tradition, emphasized the importance of fiscal policy and assigned to money a secondary and mostly accommodating role. The Report reflected a strong scepticism at the time of the effectiveness of monetary policy. It emphasized the liquidity of the economy as the key variable for monetary analysis and policy and thus represented the disbelief of traditional Keynesianism towards money. In addition it supported the view that the investment function was interest inelastic and that therefore the connection between money and economic activity was weak. In consequence, money could not be used as a major policy instrument. The channel of influence was considered to be liquidity. Through the latter, and the interest rate, monetary actions were supposed to influence the level of demand. Since liquidity was determined by the 'willingness' of financial institutions, it could frustrate the effects of monetary actions. Finally, a causal connection between money and prices was disputed on the basis of, according to the Radcliffe Report, "unlimited velocity"<sup>13</sup>.

This, however, stimulated a great deal of criticism on the part of the Monetarists. Firstly, they claimed that the Report's contention about the interest inelasticity of investment could not be accepted as it was based on casual evidence of questioning businessmen about their beliefs of what determines their investment behaviour<sup>14</sup>. Secondly, they contended that velocity was not proved by empirical

evidence to be unstable. At the least the issue is controversial<sup>15</sup>. And thirdly, they criticised the concept of liquidity in terms of its unmeasurability. The use of the concept of liquidity could not be justified as it was impossible to pin it down empirically and therefore test its validity;

it is clearly quite impossible in principle to measure 'liquidity'. No refutable theoretical propositions can be formulated in terms of liquidity. The pure Radcliffe theory can never be tested.<sup>16</sup>

But how is it that Fiscalists, who supported the Radcliffe views, could ever adhere to such casual evidence; is it that Fiscalists at the time did not espouse the positivist principle of empirical testability? Fiscalists argued that the Radcliffe Committee based its propositions on general experience because it did not have any other choice. At the time the Report was compiled there was a lack of quantitative information. As Dicks-Miraux says,

... a note of protest should be raised at the unwarranted complaint about the Radcliffe Committee's lack of quantitative evidence. At the time, the data which the Committee had available to it was indeed sparse; . . . it did not reject a quantitative approach out of choice.<sup>17</sup>

Nonetheless it was true that the Radcliffe Report rejected the quantity theory on the basis of liquidity, and what it implies for the role of money<sup>18</sup>. Later Fiscalists, however, did not accept the rejection of Monetarism and the validity of Fiscalism on the basis of the Radcliffe Report<sup>19</sup>. As we shall see, they also provided quantitative evidence for the support of their thesis. People

belonging to the fiscalist tradition, adhere to the methodology of Positive Economics and support the validity of empirical testing. Accordingly they formulate their theoretical propositions so as to be empirically testable. They use empirical testing as a criterion of validation as Monetarists do. The only difference is the method of extracting empirical evidence. Fiscalists prefer complex-structural econometric models, whereas Monetarists prefer simple-equation estimates.

Perhaps, then, the accumulation of empirical evidence should have persuaded Fiscalists that the Committee's views were extreme. Indeed, the connection between monetary actions and economic activity was now cast in terms of the influence that money has on interest rates; also price expectations were viewed as an important factor in evaluating policy actions. Yet the main conclusions were not far removed from the Radcliffe Report. Monetary policy, although featuring with some importance, was still considered as secondary to fiscal policy. Money could not be used as a discretionary policy instrument, and that is very close to what the Radcliffe Report maintained<sup>20</sup>. Although this time the evidence was considered respectable, the controversy was not stilled. There were always qualifying statements from both groups regarding the acceptance or rejection of the evidence.

b. 1960-1970

For many economists, belonging to both sides of the controversy, this particular sub-period presented itself as a test case or as an almost precise experiment in which the conflict could have been resolved<sup>21</sup>.

In most other periods one could not evaluate the relative merit of each policy, since both groups assigned the effects to their own policy. If, for example, there was an expansionary fiscal policy and money supply was growing at an accommodating pace, then Monetarists would say that, had monetary policy not been easy it would have been doubtful whether fiscal policy would be successful. It was the same for Fiscalists, had there not been a change in government expenditure or taxes, the impact of a change on a monetary total would have been doubtful<sup>22</sup>. However, some people would argue for the possibility of discrimination between the effects of each policy in some periods. Fand, for instance, says that, ". . . it is only in such singular periods - as in 1966 and 1968 - when the monetary aggregates and the full-employment surplus move in opposite directions that we get any real test of their relative effect"<sup>23</sup>. For Monetarists, the 1968 tax-surcharge seemed to have been the most evident proof of the failure of Keynesianism to combat inflation or apply correct policies. On the other hand, for many Fiscalists this interpretation of the particular instance needed qualification and by no means represented evidence against their theory<sup>24</sup>.

To begin with, around 1962 fiscal policy seemed to have relatively stabilised the economy. This was achieved through depreciation allowances and the implementation of investment tax credit. Through this success the potential of fiscal policy seemed to have been enhanced<sup>25</sup>. Meanwhile, monetary policy played a passive, minor role allowing only for a steady increase in the money supply of about three to four per cent<sup>26</sup>. This feeling of fiscal success was

accentuated by the introduction of the 1964 tax-cut for the purposes of stimulating the economy according to the prescriptions of Keynesian theory as applied by the Kennedy administration and Walter Heller. "The capstone of postwar policy for putting the U S economy more or less permanently into the full-employment orbit was, of course, the great tax cut of 1964"<sup>27</sup>. Although the period from 1964 to 1965 seemed to have been a textbook example of the success of fiscal policy<sup>28</sup>, Monetarists insisted that this particular period did not prove the effectiveness of fiscal policy, neither did it discredit the monetarist thesis<sup>29</sup>.

Despite the monetarist criticism, however, 1964 to 1965 was generally considered a low point for Monetarism. This period was the test case of Keynesian policy and it was considered successful.

But the joy was not going to last for long; the so-called 'credit-squeeze' produced a 'mini-recession'. This slackening of the economy was associated with monetary restraint and eventually people came to recognise the effects of monetary policy as an independent instrument. In addition, because government expenditure at the time was stimulative whereas the money supply fell, the importance of the role of money in affecting the economy was clearly seen<sup>30</sup>. This recognition resulted in an 'easy money' policy with the purpose to stimulate the economy. The success of monetary policy was widely recognised - ". . . This episode" says Klein "was the first in the era since World War II in which monetary policies were predominant in shaping economic movement"<sup>31</sup>. This policy success, along with the

failure of the British 1967 devaluation, contributed to the beginning of the downfall of Fiscalism and the emergence of Monetarism as a "counter-revolution"<sup>32</sup>. The consequence was that, although Fiscalists began to accept monetary policy, they did not agree with the implicit discrediting of fiscal policy; thus the controversy was highly intensified<sup>33</sup>.

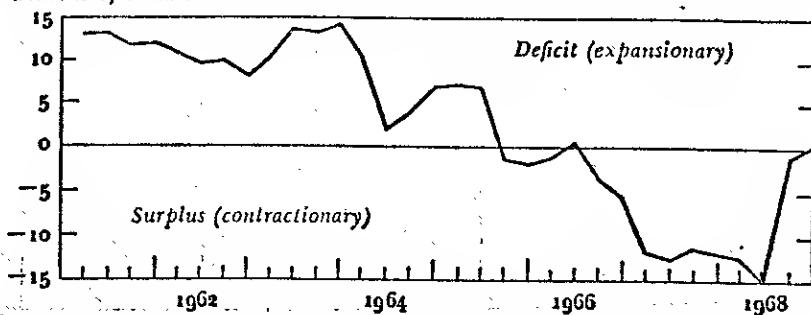
The 'high-tide' (this time of Monetarism) continued to grow with the time when the American government in June 1968 passed a law for a massive tax increase in order to 'cool off' the overheated economy. The purpose of the tax-surcharge was to reduce aggregate demand according to the policy implications of Keynesian theory and gradually bring down the price level. However, it did not prove to be successful - it was also considered bad timing due to the fact that the effects of the policy coincided with the escalation of the Vietnam war and the consequent expansion of government expenditure<sup>34</sup>. Monetarists attributed this failure to the relatively easy money policy and the decrease of the discount rate in August 1968. The result, at least according to the Monetarists, was that since prices continued to increase fiscal policy was ineffective, and that monetary changes had the most effective impact. According to Darryl Francis,

This evidence, . . . i.e., the failure of fiscal restraint which began in mid-1968 - a time when money continued to increase at an excessive rate - . . . demonstrates that monetary actions measured by changes in the money stock should receive the main emphasis in economic stabilisation<sup>35</sup>.

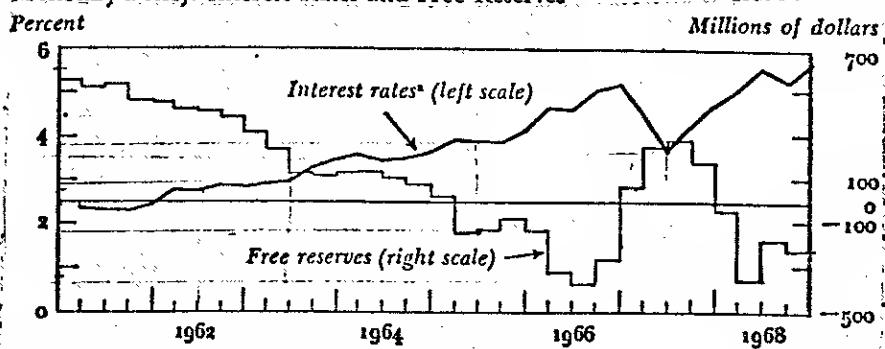
(For the development of the Policy instruments in this period see Figure IV.)

FIGURE IV: POLICY INSTRUMENTS

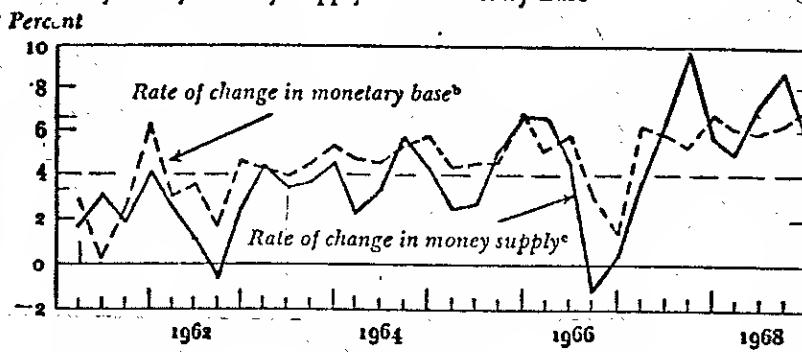
**Fiscal Policy: The High-Employment Budget**  
*Billions of dollars*



**Monetary Policy: Interest Rates and Free Reserves**



**Monetary Policy: Money Supply and Monetary Base**



Notes:

- a. Yield on 3-month Treasury Bills.
- b. Member Bank Reserves plus currency outstanding.
- c. Currency plus demand deposits.

Source: Bach, "Making Monetary and Fiscal Policy", op.cit., p.147.

Many Fiscalists, however, challenged this view mainly on three grounds. Firstly, they argued that either the tax was temporary and according to modern consumption theory, postulating the Permanent-Income hypothesis, any temporary tax-change should have only a minor effect on aggregate spending<sup>36</sup>. Secondly, they claimed that the conception of the Keynesian model as a tax-change-aggregate demand relationship was too naive and that the effect should be studied within a structural model proper<sup>37</sup>. And thirdly, that at least certain categories of spending were affected by the imposition of the tax-surcharge<sup>38</sup>.

In so far as the Monetarists criticised the Fiscalist's failure of predicting the movement of the price-level, Fiscalists argued either in terms of the inflationary expectations people held offsetting the tax-effect or in terms of, as Klein argues, estimation errors. In fact they claimed that although fiscalist models tended to underestimate price changes it was also true that monetarist models did not predict price movements accurately either<sup>39</sup>. In conclusion Fiscalists in fact never admitted the 'decisive' proof of the test of fiscal policy within this period<sup>40</sup>.

Despite this defence of Fiscalism it can still be maintained that for most economists the 1968-1969 period was interpreted as the turning point in the ascent of Monetarism. Most Fiscalists started to think of Monetary policy as an effective instrument, although only secondary to budget manipulation<sup>41</sup>.

These periods, therefore (1964-1966 and 1968-1970) are considered to embody two presumably fairly exact experiments to which both schools can refer for the truth of their theory. Surely some economists could refer also to other periods; yet the above two periods are accepted by both groups as being clear-cut examples and indications of the effectiveness of their own policy.

It is along these lines that the controversy developed. Economists waited for the time whereupon they could interpret the success or failure of a particular economic policy as evidence for the support of their own theory and a rejection of the other's. For the Fiscalists, on the one hand, the 1964 success of the fiscal stimulus was considered a doubtless proof of Fiscalism. An accommodating monetary policy was considered important<sup>42</sup>, but this was a far cry from what Monetarists thought monetary policy to be. For Fiscalists it was rather obvious that a non-discretionary exercise of monetary policy, in the form of a steady increase in the money supply, as Monetarists indicated, would prove inapplicable<sup>43</sup>. What was needed was control of aggregate economic activity through fiscal management; and the 1964 expansion verified this view. It also proved that what Monetarism proposed was indefensible. As Heller says,

... surely, the monetary policy prevailing at the time (which allegedly did the whole job) had no such power. The tax cut was the critical motive force . . . Both in the breach and in the observance, fiscal policy demonstrated its potency during the 1960's.<sup>44</sup>

Moreover, in discussing evidence for the truth of Fiscalism he says that,

... even apart from the impressive correlation between fiscal activism and high employment, we have also had a series of experiences on the firing line in which the predicted consequences of specific fiscal actions . . . became the actual consequences.<sup>45</sup>

For the Monetarists, on the other hand, the 1966-1968 period was considered a dramatic experiment<sup>46</sup> and a decisive proof of their thesis. The fact that, in the first part of the period, fiscal policy was stimulative while there was a monetary restraint producing a 'mini-recession', coupled with the case, in the second part of the period, where monetary ease produced inflationary pressures, showed, for them, beyond any doubt, the correctness of the monetarist propositions. It also showed that Fiscalism was ineffective.

There are two important pieces of evidence supporting the monetary view. One is the mini-recession experience following the monetary restraint of 1966 . . . The other one is the failure of fiscal restraint which began in mid-1968 - a time when money continued to increase at an excessive rate.<sup>47</sup>

The difficulty, however, was that although everybody accepted success of policy as evidence of the truth of their hypothesis, it was only the opposing group that presented failure of policy as evidence for the refutation of the hypothesis. For failure indicated that, for either group, things had not remained the same. According to Heller,

In 1966-68, Vietnam escalation coupled with initial Presidential hesitation to ask for a tax boost and later Congressional delay in enacting one, led to the opposite result . . .<sup>48</sup>

On the part of Monetarists Friedman argued that,

so far as I know, there has been no empirical demonstration that the tax cut had any effect on the total flow of income in the U.S. There has been no demonstration that if monetary policies had been maintained unchanged . . . the tax cut would have been really expansionary on nominal income.<sup>49</sup>

However, as far as 'proving' the theories was concerned, as was shown, 'things' were interpreted as if they had remained the same. Monetarists and Fiscalists, being members of the same neoclassical paradigm, and aspiring to positivist principles, use ceteris paribus clauses to qualify their theories. Yet these clauses must equally apply to both cases of verification and refutation. Nonetheless, economists in the controversy used them exclusively to qualify the opponent's criticisms while disregarding them in their exclamations about the truth of their theories. The application of ceteris paribus, in order to be consistent must equally apply to successful theories. As Coddington says, "why a theory 'works' is just as problematic as why it fails to work"<sup>50</sup>. 'Things', in a case whereby a theory is verified must also be shown to have remained the same; otherwise the proof of the theory may not be considered as valid, since 'things' might have already changed<sup>51</sup>.

The obvious question to ask is whether one can check the opponent's allegations and can provide an objective standard for evaluating alternative interpretations. Indeed one could argue, probably a Positive economist would so argue, that falsification or verification do not depend on ad hoc observations or policy outcomes, but rather require testing with precisely measured empirical facts.

However, Monetarists for instance, although being representatives of Positive economics, used this type of ad hoc evidence, calling it 'experience', and added it to the 'harder' types. Although they would be careful in overstressing the validators power of this type of evidence they would, nonetheless, contend that, "The most important element [of experience] was / widespread disillusion with the predictions derived from Keynesian theory . . . the failure of post-war depression to occur . . . failure of cheap money policy . . ."<sup>52</sup>. On the part of Fiscalists Heller also claims that "we have to look at specific economic experience for cause-and-effect sequences that demonstrates the potency of fiscal policy" and he goes on to present a list of episodes that, presumably, prove his point<sup>53</sup>. Friedman, although he is more reluctant about the use of 'episodic' experience as evidence, still maintains that "1966-67 is a nice episode. It is a nice controlled experiment"<sup>54</sup>, or that "The Great Contraction is tragic testimony to the power of Monetary policy".<sup>55</sup>.

Even though this type of "experience" is used as one type of evidence, albeit an ad hoc one, to be added to the rest of the types of evidence in use for the process of theory validation, it is rather an elusive type which cannot claim great objectivity and be called a 'fact'. As was shown, neither Monetarists nor Fiscalists accepted the conflicting evidence presented by the opposing group as sufficient to reject their own theory or accept the opponent's. Fiscalists, in fact, may have accepted the importance of money. But this was far from what Monetarists tried to prove as their hypothesis. For them the important thing was non-intervention of any sort. If the money

supply was allowed to grow at a certain percentage every year, the price system would work itself out of crises or rigidities. Fine-tuning, according to Monetarists, was impossible<sup>56</sup>. On the other hand, for Fiscalism, the issue was never about whether money mattered or not; money was considered unimportant only in such cases as the Radcliffe Report; for Fiscalists of the 'mixed-economy' type, the hypothesis was of discretionary policy of both monetary and fiscal actions<sup>57</sup>. What follows from this is, that 'facts', such as the interpretation of experience from alternative policy impacts, did not succeed in persuading either Monetarists of the power of fiscal policy, or Fiscalists of the importance of monetary policy cast in terms of a fixed rule.

In consequence economists from both sides had to appeal to more convincing evidence. More than ad hoc facts were needed and either group had to resort to more reliable sources to test their theories. Objectivity had to be determined with exact, measurable, empirical experience. After all that is what Positive economics is all about. After a brief consideration of the theoretical arguments in the controversy, I shall attempt to show that even with this 'harder' type of evidence very little was achieved in coming closer to a resolution of the Monetary debate.

## FOOTNOTES

## PART II:

## OUTLINE:

1. "Keynesian Monetary Theory and the Cambridge School", in Johnson and Nobay, Issues in Monetary Economics, op.cit., pp.6-7.
2. They think they stay within Positive economics because the rules are ambiguous and fluid and, as we shall see, allow a margin of adjustment.

## CHAPTER 3:

3. In addition to the Keynesian failures the monetarist 'counter revolution' was stimulated by the intensive empirical research undertaken by the Chicago School led by Friedman and by the St. Louis Federal Bank led by Andersen, Jordan, Brunner, et al. Johnson in his "Keynesian Revolution and the Monetarist Counter Revolution", (in his Further Essays in Monetary Economics, 1972, pp.50-69) argues that the Monetary 'counter revolution' was made along the Kuhnian path of scientific revolutions, the orthodox paradigm being Keynesianism established in the thirties. However, a Kuhnian paradigm entails a clearly stipulated methodology which is toppled by an alternative paradigm embracing a new and incompatible methodology. As we shall see, both Monetarism and Keynesianism are methodological variations of the same Positive economic theme, embracing common methodological (i.e. Positivism) and ontological (i.e. neoclassical theory) assumptions. Two incompatible paradigms in economics, that can be compared to the Kuhnian paradigm for example, are Classical Political Economy and Positive economics.
4. Monetary policy for the Monetarists means the direct control of the money supply, somehow defined, and not the control of the interest rate as it is implied by Keynesianism.
5. The term 'Keynesian' needs, I think, some clarification. There are possibly three factions that can claim the name 'Keynesian'. One is the English school (Coddington, perhaps more appropriately, uses the term "program" instead of theory or school to designate the various schools of economic management, "Rethinking Economic Policy", Political Quarterly, 1974, pp.430-1), spurred by the writings of Hicks and to an extent Joan Robinson; the second is the American school, led by Tobin, Heller, Samuelson, et al., and spurred by the writings of Lekachman and Hansen; and the third is the school that follows Leijonhufvud's interpretation of Keynes. In view of this, it is necessary to separate the various 'Keynesian' traditions and give them separate labels (see A. Coddington, "Keynesian Economics: The Search for First Principles", Journal of Economic Literature, 1976). As far as this analysis is concerned I shall label as Keynesians those who

(cont.)

## FOOTNOTES (cont.)

## PART II: CHAPTER 3:

5. (cont.) adhere to the first and third schools and Fiscalists those who adhere to the second one (what Coddington has labelled the "hydraulic" school, *ibid.*, pp.1263-7). It is quite important to distinguish between Keynesians and Fiscalists because two different theoretical and policy perceptions are involved. As far as Keynes himself is concerned it seems that his original theory is closer to Monetarism than later Keynesianism or Fiscalism. As Johnson confirms, "The neo-quantity theorists can dispute the credentials of the contemporary Keynesians [meaning Fiscalists], and claim the master's blessing for their own endeavours", ("Keynes and the Keynesians" in his "Further Essays . . .", *op.cit.*, p.72). It seems that concepts used by Keynes, such as expectations and wealth effects, are closer to monetarist concepts. According to Johnson, "Keynes was wrestling with basic theoretical problems of a kind which have attracted in recent times, the attention of the neo-quantity theorists rather than the Keynesian model builders", (*ibid.*, p. 72). As far as the Monetary controversy is concerned, the battle is fought between Monetarists and Fiscalists. Strictly speaking the controversy is not over Keynes' theory against some version of the Quantity Theory of money but rather over the simplifications and interpretations of Keynes' theory and policy. In this sense, therefore, whenever a fiscalist hypothesis is rejected by Monetarists this does not imply the refutation of Keynesian theory. It rather refers to the body of theories and policies formulated by (hydraulic) Fiscalists over the post-war years.
6. H.G. Johnson, "Recent Developments in Monetary Theory - A Commentary", in D.R. Croome and H.G. Johnson, Money in Britain 1959-1969, 1970, p.83.
7. I say apparent because Friedman has allegedly established that the interpretation of the facts of the 1929 crisis was wrong. The Economic Depression was not due to the inability of monetary policy to act, as Keynesians in general claim, but because of the inertia of the monetary authorities and the ensuing panic. (See M. Friedman, "The Role of Monetary Policy", in W.L. Johnson and D.R. Kamerschen, Macroeconomics, 1970, p.372, see also H.G. Johnson, *ibid.*, p.88.)
8. H.G. Johnson, Essays in Monetary Economics, Second Edition, 1969, p.245.
9. Yet even in this Friedman sees a failure of the Keynesian theory. He claims that Keynesians after the last war predicted a sharp economic depression which, in the end, never appeared (see his Money and Economic Development, 1972, p.5). Nonetheless, people like Walter Heller heralded this period of economic expansions as due, primarily, to fiscal policy (see Heller-Friedman, Monetary Versus Fiscal Policy, 1969, p.30).

## FOOTNOTES (cont.)

## PART II: CHAPTER 3:

10. See, "The Quantity Theory of Money: A Restatement", in his "Studies in the Quantity Theory of Money", op.cit., pp.51-67.
11. See, A.A. Walters, "The Radcliffe Report - Ten Years After: A Survey of Empirical Evidence", in Croome and Johnson, "Money in Britain", op.cit., pp.39-68.
12. Ibid., p.40.
13. See, V.E. Morgan, "The Radcliffe Report in the Tradition of British Official Documents", in Croome and Johnson, op.cit., pp.10-11.
14. Walters, ibid., p.40.
15. Ibid., p.41.
16. Ibid., p.40.
17. L. Dicks-Mireaux, "Discussion Paper", in Croome and Johnson, "Money in Britain", op.cit., p.69.
18. Morgan, ibid., p.40.
19. See, P.A. Samuelson, "The Role of Money in National Economic Policy", in Controlling Monetary Aggregates, 1969, p.9.
20. Morgan, ibid., p.11.
21. B.W. Sprinkel, Money and Markets. A Monetarist View. 1971, p.8. Also, K.M. Carlson, "Monetary and Fiscal Actions in Macroeconomic Models", Federal Reserve Bank of St. Louis Review, 1974, pp.9-10.
22. Friedman-Heller, "Monetary Vs Fiscal Policy", op.cit., p.32 and pp.56-57.
23. D. Fand, "Monetarism Vs Fiscalism", Banca Nazionale del Lavoro, 1970, p.291.
24. R. Teigen, "A Critical Look at Monetarist Economics", Federal Reserve Bank of St. Louis Review, Jan.1972, p.10. Also, Klein, "Empirical Evidence on Fiscal and Monetary Models", in Diamond, "Issues in Fiscal and Monetary Policy: The Eclectic Economist Views the Controversy", op.cit., pp.36-42. Also, Heller, in "Friedman-Heller", Monetary Vs Fiscal Policy, op.cit., pp.35-36.
25. The investment tax credit in fact heralded the beginning of active fiscal policy. See G.L. Bach, Making Monetary and Fiscal Policy, 1971, pp.114-116.

## FOOTNOTES (cont.)

## PART 11: CHAPTER 3:

26. The period from 1961 to 1953 experienced a 'mini-recession' with unemployment around 5% and G N P declining. The government surplus increased and the ensuing "fiscal-drag" contributed to the recession. This stimulated the Kennedy administration, with the help of the "New Economics", to produce a substantial tax-cut to alleviate the surplus and boost the economy. See Bach, ibid., p.114.
27. Heller, "Fiscalism Vs Monetarism", op.cit., p.32.
28. Johnson in fact admits that ". . . the tax-cut has been . . . a . . . step toward the efficient use of fiscal policy as a major instrument of domestic economic stabilisation". "Essays in Monetary Economics", op.cit., p.246.
29. See, Sprinkel, "Money and Markets", op.cit., pp.8-9.
30. See H.G. Johnson, "Monetary Theory and Monetary Policy", in his "Further Essays in Monetary Economics", op.cit., p.81.
31. Klein, "Empirical Evidence . . .", op.cit., p.37.
32. Johnson, "The Keynesian Revolution and the Monetarist Counter-Revolution", op.cit., pp.57-60.
33. "One can join the chorus of critics of 1966-68 policy without accepting the gloomy inference for the future . . . of discretionary policy . . .", Heller, in Friedman-Heller, "Monetary Vs Fiscal Policy", op.cit., p.35. See also, Klein, "Empirical Evidence on Fiscal and Monetary Models", op.cit., p.40.
34. See, Johnson, "The Keynesian Revolution . . .", op.cit., p.58.
35. "The New, New Economics and Monetary Policy", Federal Reserve Bank of St. Louis, January 1970, p.6.
36. Teigen, "A Critical Look at Monetarist Economics", op.cit. p.10.
37. Ibid., p.10.
38. Ibid., p.11.
39. Klein, "Empirical Evidence on Fiscal and Monetary Models", op.cit., p.40.
40. "This experience . . . was not viewed as a defeat for fiscal policy and the Keynesian model. Rather, the experience suggested that the economy had moved into the 'classical range' of the LM curve - the range in which monetary actions have their greatest potency relative to fiscal actions." Carlson, "Monetary and Fiscal Actions in Macroeconomic Models", FRB of St. Louis Review, January 1974, p.11.

## FOOTNOTES (cont.)

## PART II: CHAPTER 3:

41. Samuelson, in fact stresses this point by saying that, "There is nobody, I think, worth our notice on the American scene who [believes that] . . . money doesn't matter . . .", Samuelson, "The Role of Money in National Economic Policy", in "Controlling Monetary Aggregates", op.cit., p.7. Also in the Annual Report of the Council of Economic Advisers in 1967, they admitted that, "the power of tight money as a tool of restraint . . . was demonstrated beyond any reasonable doubt", quoted in Carlson, "Monetary and Fiscal Actions in Macroeconomic Models", op.cit., p.10.
42. Heller, "Monetary Vs Fiscal Policy", op.cit., p.33.
43. Ibid., pp.33-35.
44. Ibid., p.67 and p.32.
45. Ibid., pp.65-66.
46. Friedman, "Monetary Vs Fiscal Policy", op.cit., p.57.
47. Francis, ibid., p.6. See also Fand, "Some Issues in Monetary Economics", op.cit., p.18 and p.25, Carlson, "Monetary and Fiscal Actions . . .", op.cit., pp.11-12, and Friedman, "Monetary Vs Fiscal Policy", op.cit., pp.57-8.
48. Heller, ibid., p.31. See also, Teigen, "A Critical Look . . .", op.cit., p.10, and J. Duesenberry, "Tactics and Targets of Monetary Policy", in Controlling Monetary Aggregates, I, 1969, op.cit., p.83.
49. Friedman, ibid., p.55. Also Fand while referring to Okun's analysis of the tax cut says that, "[It] explicitly justifies the omission of any capital market or monetary effects. Although Okun accepts the view that significant changes in the cost or availability of credit would have an important influence on business investment, he does not make allowance for these factors in his quantitative estimates of the multiplier", "Some Issues in Monetary Economics", Federal Reserve Bank of St. Louis Review, January 1970, p.24.
50. "Rethinking Economic Policy", op.cit., p.432.
51. For instance, Leeman observes that, "When facts disagree with their conclusions, they [economists] point out that their laws are only tendencies, that of course they are assuming that other things remain equal, etc. But when facts are in agreement with their generalizations, economists are quick to appeal to them as evidence and as verification of the generalizations which have been put forth", W.A. Leeman, "The Status of Facts in Economic Thought", The Journal of Philosophy, 1951, p.409.

## FOOTNOTES (cont.)

## PART 11: CHAPTER 3:

52. Friedman, "Money and Economic Development", op.cit., pp.4-5, see also Brunner's quotation on p.126 in the following chapter.
53. Heller, "Monetarism Vs Fiscalism", op.cit., p.31.
54. Friedman, "Monetarism Vs Fiscalism", op.cit., p.31.
55. Friedman, "The Role of Monetary Policy", op.cit., p.372. Also Sprinkel adds that, "Fortunately for science as well as for future policy makers and investors, four episodes in the years spanning 1966-70 permit a reasonably definitive post-mortem evaluation. Of course, it is impossible to prove that either view is the 'ultimate truth'. Yet the evidence during this time period is highly suggestive". However, although he qualifies this by saying that this evidence constitutes "a very small part of the total evidence" he still considers it as legitimate evidence; "Money and Markets", op.cit., p.8 and p.16.
56. Friedman, "Monetarism Vs Fiscalism", pp.47-48.
57. Samuelson, "The Role of Money in National Economic Policy", in Controlling Monetary Aggregates, I, 1969, op.cit., p.7 and p.9.

CHAPTER 4

T H E   T H E O R E T I C A L   F R A M E W O R K  
O F   T H E   C O N T R O V E R S Y

## CHAPTER 4:

THE THEORETICAL FRAMEWORK OF THE  
CONTROVERSY

## A. INTRODUCTION

Policy actions and their success or failure form part of the 'experience' package used by Monetarists and Fiscalists for theory validation. For instance, Brunner comments:

What evidence may be cited on behalf of the . . . Monetarist thesis? Every major inflation provides support for the thesis, particularly in cases of substantial variations in monetary growth . . . The association between monetary and economic accelerations or decelerations has also been observed . . . Observations from periods with divergent movements of monetary and fiscal forces provide further evidence.<sup>1</sup>

Regarding inflation, however, we have seen in the previous chapter that it is a phenomenon subject to wide interpretations when presented as evidence for the support of either Fiscalism or Monetarism. Neither faction accepted the rival interpretations made of these periods. For example, Modigliani exclaims that, "Indeed, when I look at the recent inflationary experience going back to 1966, I find it hard to decide whether the prize for misbehaviour should go to our fiscal or to our monetary policy makers"<sup>2</sup>. Regarding the causal link in the association between money variations and income it is also an issue disputed quite strongly by Fiscalists, the strongest position possibly taken by Kaldor in his controversial article, "The New Monetarism"<sup>3</sup>. Finally as far as the last point in the quotation is concerned, Brunner mentions "divergent periods" implying the 1964 and 1968 periods in American economic policy. However, we have seen that although it is true that

the two policy trends moved in opposite directions, one could not deduce from this that they were also objective experiments or conclusive tests of the truth of either theory, since neither Fiscalists nor Monetarists were convinced of the rival interpretations. These periods constituted only the basis for qualitative judgements mostly substantiated by casual observations, such as the fact that the tax cut did not succeed in stimulating the economy, and therefore the Keynesian predictions had failed<sup>4</sup>.

Scientifically and formally, however, these types of observations constitute a minor - although admittedly a major emotional - factor and are complemented, and most of the time displaced, by empirical evidence which is assessed 'precisely' and in a 'dispassionate' way. "Intuition, experience and judgement have now been subjected to ten years' statistical and econometric analysis"<sup>5</sup>. Positive economic methodology comes to rescue the economist from the passionate and subjective ad hoc judgements and replaces them with 'hard' econometric facts. It is supposedly the accumulation of such 'facts' that provides the economist with a scientific basis against which a hypothesis should stand or fall.

But in order to be able to extract such evidence from the chaos of reality even the utmost empiricist must use some type of an ordering principle or theory in order to make meaningful classifications. As Friedman says, "Every empirical study rests on a theoretical framework, on a set of tentative hypotheses that the evidence is designed to test or adumbrate"<sup>6</sup>. The purpose of this chapter then is

to review this theoretical framework within which the controversy takes place. In doing this I shall try to clarify the two opposing positions. From this one will be able to see that the controversialists partake in the same theoretical framework, and their differences are not of kind but of degree. In other words Monetarists and Fiscalists share the same theoretical perspective, i.e. modified neoclassical theory and the general IS-LM framework, varying their positions mainly on time horizons, and where to put the emphasis within this framework. These differences, as we shall see, stem from different assumptions about the stability and rigidity of the system. In turn these assumptions are corroborated by, and interrelated to, the different methodological assumptions mentioned in the previous chapter. That is, a predictionist position, implying a simple correlational method, simultaneously justifies, and is corroborated by, the monetarist position, which implies flexibility and stability of the free market. On the other hand, an assumptionist position, implying a complex, structural approach, justifies, and is corroborated by, the fiscalist position, which implies rigidities and the instability of the economy. In short the flexibility and stability assumptions correlate with the 'unrealist' assumption, whereas the rigidity and instability with the 'realist'.<sup>7</sup>

In what follows I shall review this common theoretical framework and examine the different positions taken within it.

## B. THEORETICAL DIFFERENCES

## a. THE PRICE LEVEL

In 1956 Milton Friedman, the chief proponent of Monetarism, revived the old quantity theory of money - made redundant by the Keynesian 'revolution' - as part of what Johnson calls the "counter-revolution", and cast it into different terms. Instead of the old identity the relationship of money took the form of a behavioural function relating the demand for real balances with a few, and determinate, independent variables. One of the purposes of this redefinition was to give the quantity theory an empirical content made to fit the empirical testability requirement. In addition it enabled Monetarists to collect statistical evidence in favour of the Quantity Theory and against the income-expenditure model. In fact the debate took the empirical form of testing the stability of the Keynesian multiplier against the stability of the velocity of money, as these hypotheses were considered the empirical implications of 'alternative' theoretical frameworks.

Friedman claimed that this was a major breakthrough achieved through the continuation of an "oral tradition" kept alive in the University of Chicago while Keynesianism was in power. However, this was proved not to be true. As Patinkin has shown there never did exist such a tradition<sup>8</sup>. In fact he says that,

what Friedman has actually presented is an elegant exposition of the modern portfolio approach to the demand for money which, though it has some well known . . . antecedent in the traditional theory, can only be seen as a continuation of the Keynesian theory of liquidity preference.<sup>9</sup>

This much was also admitted by Friedman who acknowledged the fact that the reformulation of the quantity theory was "much influenced by the Keynesian liquidity analysis"<sup>10</sup>.

However, Friedman in a subsequent article suggested that although it is granted that the theoretical framework is similar to the Keynesian there is one important difference and this concerns the "missing equation"<sup>11</sup>. Friedman sets out the familiar IS-LM model<sup>12</sup>:

$$(1) \quad \frac{Y}{P} = C\left(\frac{Y}{P}, r\right) + I(r) \quad (\text{equilibrium in the market for goods and services})$$

$$(2) \quad M_o = P \cdot L\left(\frac{Y}{P}, r\right) \quad (\text{equilibrium between the demand for real balances and the real value of the nominal stock of money})$$

$$(3) \quad Y = Py \quad (\text{definition relating money to income})$$

where:  $Y$  = nominal income,  
 $P$  = general price level,  
 $r$  = rate of interest,  
 $M_o$  = the nominal exogenously set stock of money, and  
 $y$  = real income.

In it he shows that there are three equations to determine four unknowns, namely:  $Y$ ,  $P$ ,  $r$  and  $y$ . The additional market or equation to be included constitutes, according to Friedman, the fundamental characteristic and important difference distinguishing the monetarist from the fiscalist theory<sup>13</sup>.

This difference in assumption, according to Monetarists, is that on the one hand Fiscalists following Keynes' model discuss

everything in terms of wage units and they assume a stable price level<sup>14</sup>, whereas they, on the other hand, assume Walrasian types of markets which are flexible and in which series of demand and supply equations determine the level of real output, allowing therefore for price expectations to be determined endogenously by the system<sup>15</sup>.

It has been counter-argued, however, that the above difference concerning the 'missing equation' relies heavily on a naive conception of the Keynesian model. According to this argument, Monetarists use as a model exposition of Keynesianism the one version of Keynesian theory relating money, interest rate, output and the labour market, as it is simplified in the Hicksian cross and which at a certain level assumes a stable price level<sup>16</sup>. Or Tobin would argue that, "Milton Friedman's contention that the crucial difference between Keynesians and the Monetarists is that Keynesians assume rigid prices is shown to be factually and logically wrong"<sup>17</sup>. And yet Friedman goes on to insist that, "Whatever the [Keynesian] group may say in their asides and in their qualifications, they treat the price level as an institutional datum in their formal theoretical analysis"<sup>18</sup>. But what are these qualifications and how important are they? Some Fiscalists would argue that they are not qualifications at all but they are different assumptions indeed, however not about the price level but about the complete determination of the system. They insist that what they assume as rigid is not every price but only the price of labour, i.e. wages<sup>19</sup>. This assumption comes into contrast with Monetarism because the latter assumes a process of *tâtonnement* in Walrasian flexible markets. Therefore the difference in reality lies

in the assumption about rigidity in the labour market and not in the difference about price expectations as most Monetarists have it<sup>20</sup>.

In particular the Keynesian interest lies in the institutionally caused disequilibrium in the labour market in which "there is no equilibrium . . . but rather a dynamic wage-adjustment equation determining the rate of change of the nominal wage rate in response to the state of excess supply in the market"<sup>21</sup>.

So, apparently, there is a difference between Fiscalists and Monetarists, namely that of Walrasian flexibility and full employment on the one hand, and institutional disequilibrium in the labour market on the other. Yet how important this difference is is shown by the fact that a Walrasian system may change into a Keynesian one as soon as one takes into account information costs explicitly. "To make the transition from Walras' world to Keynes' world," Leijonhufvud maintains, "it is thus sufficient to dispense with the assumed tâtonnement mechanism [and assume information costs] . . . No other 'classical' assumption need be relinquished"<sup>22</sup>.

Moreover, Fiscalists have included in their analysis and in their construction of large econometric models price expectations explicitly (in the form of a Phillips curve) proving that changes in the price level may be included and be compatible with Keynesian theory<sup>23</sup>. The difference thus seems to stem from a difference at the methodological level, at which Assumptionists-Fiscalists assume a 'realistic' position incorporating some aspects of the 'real' world, like rigidities in the labour market, or other markets, whereas

Predictionists-Monetarists assume an 'unrealistic' (in the sense of chapter 1) position whereby complete abstraction is preferred, and absolute market flexibility is assumed. These beliefs are supported, on the fiscalist side, by the proposition that the economy is basically unstable and, on the monetarist side, by the proposition that the economy is inherently stable. As Andersen and Carlson contend, "The private economy is inherently stable . . . [and] that monetary and fiscal actions are a source of instability"<sup>24</sup>, and as Ando replies, "We believe, on the other hand, that the economy is sufficiently unstable . . . so that it requires active fiscal and monetary policies . . ."<sup>25</sup>. Thus, flexibility, i.e. individual freedom of the economic agents and institutions to act in a rational (maximizing) sense, is facilitated by stability in the economy, i.e. fairly consistent information about economic indicators. Whereas, inflexibility, i.e. hampered action of the agents, is corroborated by the belief that the economy is unstable and requires an authority to alleviate the rigidity and instability.

However, although differences between Monetarists and Fiscalists can be mostly reduced to the methodological level (i.e. Assumptionism versus Predictionism), and the consequent difference in the assumption about the structure of the economy (i.e. flexible versus inflexible markets, stable versus unstable economy), it seems that Monetarists conceive or interpret Fiscalism as representing a price-rigidity position. Despite Fiscalists' qualifications and protests about this assumption, Monetarists form their criticism according to this interpretation. For example, a consequence of the 'price

'rigidity' assumption made by Fiscalists, according to Monetarists, is that fiscalist monetary policy, conducted through interest rate indications, is considered improper and biased. The reason behind this criticism is that built-up price expectations in the interest rate are unaccounted for due to the 'fixed price' assumption rendering movements of the interest rate ineffective for correct indications of monetary policy<sup>26</sup>. Consequently if interest rates are not reliable indicators then one has to turn to the money supply, or some other kind of monetary aggregate, e.g. the monetary base, in order to take appropriate steps for monetary policy<sup>27</sup>. In effect their argument runs in the following way: at the beginning changes in the money supply might induce opposite changes in the interest rate, as is expected by fiscalist theory. However, if one assumes fast economic growth then spending and incomes will rise, raising also the liquidity preference function and thus lowering the real money supply and therefore increasing prices and interest rates. The process will reverse the initial relation bringing about a positive relation between money stock and interest rate changes.

Insofar as the above refers to naive fiscalist policy it is true. However, according to Fiscalists, it is false insofar as it refers to large and sophisticated models. The latter explicitly include price expectations and therefore take into account price-induced changes in interest rates. Furthermore the above 'money growth' effects, summarized by Friedman in "The Role of Monetary Policy"<sup>28</sup> are shown by Fiscalists to be either included in, or be consistent with, Keynesian theory. Teigen, for instance, feels that

this is not an accurate description of fiscalist policy and stresses that often,

Keynesians are supposed to recognize only . . . an inverse relationship between monetary impulses and interest rate changes. This is certainly not the case. When the entire structure is taken into account, rather than only the liquidity preference function, the level of interest rates . . . is determined by a number of elasticities . . .<sup>29</sup>

In effect, according to this argument, it depends upon the magnitude of the systemic elasticities whether the relationship is inverse or not. If for certain categories of expenditures and periods the elasticities happen to be such that the relationship is direct, then there is nothing imperative in the complete Keynesian system that prohibits or excludes it.

A further consequence of the 'price rigidity' assumption attributed to Fiscalism, relates to the so-called stability of the demand for money function and of velocity. For Monetarism, by virtue of its assumption about flexibility in the labour market, full employment will produce the stable velocity needed by Monetarism. On the other hand Fiscalists due to price fixity will hold for unstable velocity and therefore will consider money supply changes as not having a direct effect on income. According to Fiscalists, however, as soon as the price rigidity assumption is removed and the theoretical differences between the two groups are narrowed down to differences about condition in the labour market, then the solution

of the complete Keynesian system will contain both cases, i.e. depending on the numerical value of the relevant elasticities velocity is either stable or unstable. So, in some sense, the monetarist case, according to Fiscalists, is subsumed under the general and 'sophisticated' Keynesian model<sup>30</sup>.

Furthermore, concerning the monetarist allegation that Fiscalists "frequently substitute nominal variables in lieu of real variables", whereas Monetarists, "distinguish between monetary and real variables"<sup>31</sup>, it can also be maintained that it is inapplicable as soon as the price rigidity assumption is relaxed.

To recapitulate the various points made so far in this section: the theoretical difference relating to the assumption about the 'missing equation', or the exogeneity of the price level, was found not to be accepted by the fiscalist account of the 'sophisticated' Keynesian model<sup>32</sup>. The usual theoretical differences discussed in the debate literature, e.g. interest rate expectations, stability of velocity, real and nominal variables, were also shown not to be accepted by Fiscalism, on the grounds that they are variants of the price rigidity assumption. Since Fiscalists contend that the price level is determined endogenously in their system, then these criticisms or differences cannot hold true. Then what is the controversy all about? In my opinion differences between the two factions concern methodological and ontological issues taken from within the same theoretical framework. By methodological issues I mean the 'unrealistic' (i.e. simple-correlational)

versus the 'realistic' (i.e. complex-structural) approaches, and by ontological issues I mean the different assumptions the two factions entertain about the world (market), i.e. the assumptions about flexibility-stability and inflexibility-instability. In fact the methodological and ontological issues are related, in that the 'unrealistic' approach favours a flexible-stable world, whereas a realistic one favours an inflexible-unstable one. A flexible-stable world, in turn, favours the assumption of unimpeded maximization that helps run things smoothly, whereas an inflexible-unstable one favours the 'realistic' assumption of rigidities that impede maximization. For example Mundell in making a generalized exposition of Keynesian theory talks of four cases,

in which (a) prices and wages are both flexible, (b) prices are flexible and wages rigid, (c) wages are flexible and prices are rigid, and (d) prices and wages are both rigid,

and concludes that "The distinction between the four cases hinges on whether or not firms are prevented from maximizing profits and whether or not workers are impeded in their pursuit of maximum utility"<sup>33</sup>. The assumption about price flexibility therefore relates to more fundamental assumptions belonging to the same neoclassical, positivist, framework.

#### b. THE TRANSMISSION MECHANISM

Another allegedly important difference that separates the Fiscalists from the Monetarists relates to the way monetary impulses transmit their effects throughout the economy. In fact it is argued that it is more important than the 'missing equation' difference since

it involves the definition of the concept of money. It is essential for the success of a monetary policy to delineate what is the true empirical content of money so as to determine its close substitutes in order to see whether the effects of the policy are offset or not. If for example alternative financial assets are close substitutes for money balances then monetary policy may be ineffective. "The crucial distinction between the Monetarists and the Keynesians" it is argued "resides in their widely differing view of the degree to which certain alternative financial assets may be close substitutes for money balances"<sup>34</sup>. However, as we shall see this "widely differing view" is also a variation on the price rigidity theme.

To determine first the definition of money and second the channels through which it transmits its effects, there is a need for a theory of the transmission mechanism. For years the usual Keynesian criticism against Monetarism has been the one related to the "black-box" theory of the transmission mechanism. For instance Samuelson has claimed that the monetarist position "is a 'black-box' model in which money enters at the one end as an input and, for reasons unexplained and not necessary to explain, invariably induces at the other end of the box an output response in G.N.P."<sup>35</sup>. By a 'black-box' it was meant that Monetarists accept a link which does not have to be explained between money supply variations and changes in money income, whereas for their part Fiscalists claim a portfolio adjustment process through which monetary impulses transmit their effects. However, to this accusation Monetarists answer with surprise that,

It is a minor mystery in the recent history of economic thought as to where so many economists have got the impression that Friedman's view of the monetary mechanism is crucially dependent upon some mysterious 'direct' effect of money on expenditure . . . The transmission mechanism is spelled out clearly enough in more than one place, contrary to what is all too frequently alleged . . . it is an interest rate mechanism . . . and as Kaldor notes apparently with surprise is not significantly different from that described by Keynes<sup>36</sup>. (my emphasis.)

Moreover, not only did the Monetarists try to convince the Fiscalists that their propositions reflected a distinct theory of the transmission mechanism<sup>37</sup>, but also they contended that their theory was a far more general and superior theory than the Fiscalists'. Essentially they maintained that for the fiscalist theory "all information bearing on the transmission of monetary impulses is contained in the slope properties of the IS-LM diagram"<sup>38</sup>, and thus it is a restricted theory. In a certain sense therefore the process has been reversed and now the Fiscalists are accused of an oversimplified account of the transmission mechanism. According to Monetarists, Fiscalists look at a process of adjustment that includes a narrow set of financial assets with a narrowly defined spectrum of interest rates reflecting only the 'cost of borrowing'. The monetarist position is best represented by Friedman who claims that,

The Keynesians regard a change in the quantity of money as affecting in the first instance 'the' interest rate, interpreted as a market rate on a fairly narrow class of financial liabilities . . . We, on the other hand, stress a much broader and more 'direct' impact on spending.<sup>39</sup>

But how do Fiscalists respond to this criticism? Hicks for instance argues that,

the balance sheet must be considered much more generally . . . We have a well established theory of the distribution of assets in a portfolio in which the speculation considered by Keynes appears as a special case.<sup>40</sup>

Or Teigen maintains that,

there is nothing inherent in the Keynesian system which is inconsistent with the introduction of a general portfolio adjustment transmission mechanism.<sup>41</sup>

But how is it then explained that Monetarists still believe in such an allegedly important difference and carry on their tests as if to prove the truth of the different positions? The answer lies, as we shall see, in the monetarist conception of the fiscalist view as one that contains a fixed-price assumption. "This difference in the assumed transmission mechanism" says Friedman, "is largely a by-product of the different assumptions about price"<sup>42</sup>. Thus differences on the transmission mechanism are transformed into differences about the 'missing equation'. And these latter differences are in turn transformed into the fundamental differences between the two methodologies (simplicity-complexity) and the two ontologies (flexibility-inflexibility). Any kind of rigidity in the mind of the Monetarist represents more 'realistic' analysis and the impediment of the free market, two ideas that do not match with the whole monetarist outlook<sup>43</sup>. As we shall see in the transmission mechanism expounded by Monetarists money plays a more important role than interest rates because it is the instrument that allows flexibility in the market, whereas interest rates imply direct intervention.

In analysing the way in which Monetarists view the transmission mechanism we see that the dominant factor for changes in income and prices is the adjustment process, stimulated by a discrepancy between the demand and the supply of money, affecting spending and thus prices, and consequently income. In other words the amount of money in excess in the hands of the public will be spent on both financial and physical assets thus changing the general price level and output. This latter change will ultimately bring the state back into equilibrium. The emphasis of the analysis being on both types of assets constitutes, according to Monetarists, the major difference between Monetarism and Fiscalism, as far as the transmission mechanism is concerned. In addition, the interest rate is interpreted more generally and the monetary changes affect the items of the portfolio by altering the yields both physical and financial - and therefore affect income. Furthermore, resulting changes in the price level adjust the real value of the money supply. It is this mechanism then that Monetarists propound when they talk of a more general portfolio theory of the transmission of monetary impulses, in contrast to the fiscalist 'narrow' one. In effect, according to the above, Monetarists assign a causal role to the excess quantity of money in the hands of the public, i.e. to part of the money supply.

However, against this contention Fiscalists bring forward the criticism that one cannot know a priori which is the dominant factor vested with the causal power to determine the whole process of the portfolio adjustment. Fiscalists, in contrast to what

Monetarists believe, claim that they accept a general portfolio, in which no singular factor bears a causal value. In fact they ask,

[is] it the initial increase in the money stock, the full increase, . . . the increase in bank reserves, the reduction in private bill holdings, the fall in yields, the increase in the central bank's portfolio, or some other factor which is responsible for the income change[?].<sup>44</sup>

It is important to stress the point that Fiscalists do not use only one type of asset in their portolio theory but, as we have seen (see p.163, footnote 41), a whole array of them, including financial and real assets. The difference between them and the Monetarists is in the choice of where to put the emphasis. For the Monetarists the emphasis is put on changes in the money supply, and while for the monetarist conception of Fiscalism it is the interest rate on certain assets, for the more sophisticate version of Fiscalism it is "the disturbance of a portfolio equilibrium"<sup>45</sup>. Effectively the last position is similar to the first one, since the latter considers as well a general portfolio theory. However, although for the former money is important for the latter so are other factors as well<sup>46</sup>. This difference in emphasis reflects the metodological difference of simplicity versus complex structuralism. A simple causation between money and economic activity seems sufficient for the Monetarist whereas a multidimensional analysis seems more adequate for the Fiscalist.

An important corollary of all this is the distinction between the short and the long-run. For both sides - and assuming the naive

Keynesian model for the moment - the effects of changes in the money stock are separated into a liquidity and an income effect. The first one is associated with the fiscalist and the second with the monetarist position. As a consequence the former restricts itself to the short-run Keynesian world and the latter to the long-run secular analysis. In fact this short and long-run distinction relies, on the one hand, on the fiscalist assumption of wage rigidity in conditions of under full employment equilibrium and, on the other hand, on the monetarist assumption of full employment resulting from changes in the price level in the long run. This distinction is accepted by Friedman who admits that,

changes in the quantity of money as such in the long run have a negligible effect on real income, so that non-monetary forces 'are all that matter' for changes in real income over the decades and money 'does not matter'",

but as far as nominal magnitudes are concerned

we have regarded the quantity of money . . . as essentially 'all that matters'<sup>47</sup>

So essentially the distinction is between a Keynesian short-run effect on real output and a Monetary long-run effect on nominal magnitudes.

Although most Monetarists would accept this distinction that "money-matters-most" in the long-run<sup>48</sup>, they would nevertheless allege that changes in the short-run, though not the only and major factor, is a factor that accounts "for short-run changes in both nominal and the real level of activity"<sup>49</sup>. Thus, according to Monetarists, money is more important for the long-run, but it is also relatively important

for the short-run. According to Friedman,

The central notion of monetarism is that money matters for both short-term economic fluctuations of the economy and for inflation, the trend of prices. Part of the central notion - the feature that distinguishes it most from the Keynesian approach - is that what matters is the quantity of money . . . and not interest rates.<sup>50</sup>

However, these conclusions are presumably based upon the assumption of a separate and alternative framework the distinguishing characteristic of which is the endogenous determination of the price level and the emphasis on both real and financial assets in the transmission mechanism. Yet these conclusions do not seem to be supported by Monetarists themselves when they claim a different, in terms of generality, transmission mechanism but which also includes interest rates. It has been already shown that both schools believe in an interest rate mechanism. The difference, claimed by Monetarists, is that the process needs a wider range of interest rates to include both financial and real assets; and also, they claim, for interest rates to work effectively as indicators of monetary policy, price expectations must be included. However, as far as the sophisticated version of Fiscalism is concerned, it was also shown that both qualifications are included as characteristics of the transmission mechanism. As Patinkin claims,

to accept the Keynesian conceptual framework for the analysis of the demand for money does not imply that one must reject the quantity-theory conclusions about the long-run impact of monetary changes on the economy [and vice versa].<sup>51</sup>

To a large extent both systems accept an IS-LM general framework for the transmission mechanism, modified by the emphases either school

puts on the short-run or long-run, the financial or the real magnitudes.

Thus even though the general theoretical framework is the same, the differences stem from different methodological and ontological emphases<sup>52</sup>. Whereas the Monetarists tend to prefer a transmission mechanism that designates the most important role to money supply disturbances, Fiscalists tend to construct the transmission mechanism in terms of general disequilibria in various sectors and parameters<sup>53</sup>. These views are consistent with the pairs of assumptions of simplicity-flexibility and complexity-inflexibility.

### C. CONCLUSION

The Monetary controversy at the theoretical level has been characteristic of a confusion between the two factions<sup>54</sup>. The alleged differences in the flexibility or inflexibility of prices, and thus differences about the transmission mechanism, are found not to be real differences. In so far as we are considering the debate between (the 'weak' part of) Monetarism and the sophisticated version of Fiscalism, both factions seem to belong to the generalized neoclassical framework, shifting positions only in respect (a) to methodology, i.e. to differences between unidimensional and multidimensional causation - simple and complex analysis - and (b) to ontology, i.e. to differences about more realistic assumptions about the overall inflexibility and stability of the system. Both Monetarists and Fiscalists therefore seem to be associated with the same

neoclassical framework, modified in different directions<sup>55</sup>. As we shall see, the opposite directions can be taken due to the fluidity of the Fundamental Postulates of Neoclassical Economics.

Differences, therefore, exist. And these differences pertain to different assumptions as to how to go about studying the economy and as to what is the economy itself. In order to see whether these fundamental differences - and also whether the theoretical disagreement between Monetarists and the naive conception of Fiscalism - can be resolved, I now turn to examine the empirical evidence brought in favour of, or against, either faction. Perhaps, as Sprinkel says, "A careful, dispassionate study of positive monetary economics should be capable of eliminating many disagreements . . ."<sup>56</sup>

## FOOTNOTES

## PART II: CHAPTER 4:

1. K. Brunner, "The Role of Money and Monetary Policy", FRB of St. Louis Review, July 1968, p.20.
2. F. Modigliani, "Discussion", American Economic Review Papers and Proceedings, 1975, p.179. Also Hutchison claims that "the dichotomy between positive beliefs regarding the predicted effects of policies and normative attitudes regarding alternative policy objectives, does not seem to have been applied very successfully to the rapid resolving of disagreement", "Positive Economics and Policy Objectives", op.cit., p.18.
3. N. Kaldor, "The New Monetarism", Lloyd's Bank Review, 1970, pp.1-17.
4. Friedman, "Money and Economic Development", op.cit., pp.4-5 and p.32.
5. Walters, "The Radcliffe Report", op.cit., p.64.
6. Friedman, "A Theoretical Framework . . .", op.cit., p.1.
7. "Nonmonetarists accept . . . that a private enterprise economy using an intangible money needs to be stabilized, can be stabilized, and therefore should be stabilized by appropriate monetary and fiscal policies. Monetarists by contrast take the view that there is no serious need to stabilize the economy;" F. Modigliani, "The Monetarist Controversy or, Should We Forsake Stabilization Policies", American Economic Review, 1977, p.1, see also pp.12-14. Also Leijonhufvud says that, "the model which Keynes called his 'general theory' is but a special case of the classical theory, obtained by imposing certain restrictive assumptions on the latter; and . . . the Keynesian special case is nonetheless important because, as it happens, it is more relevant to the real world than the general (equilibrium) theory". A. Leijonhufvud, "Keynes and the Keynesians: A Suggested Interpretation", in Johnson and Kamerschen, "Macroeconomics", op.cit., p.210. For a different interpretation of Keynes' differences with equilibrium theory see Coddington, "Keynesian Economics . . .", op.cit., esp. pp.1260-1.
8. D. Patinkin, "The Chicago Tradition, The Quantity Theory and Friedman", Journal of Money, Credit and Banking, 1969, pp.47-70.
9. Ibid., p.47. He also adds that "the conceptual framework of a portfolio demand for money that Friedman denotes as the 'Quantity theory' is actually that of Keynesian economics", in his "Friedman on the Quantity Theory and Keynesian Economics", Journal of Political Economy, 1972, p.883.

## FOOTNOTES (cont.)

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10. M. Friedman, "Post-War Trends in Monetary Theory and Policy", in The Optimum Quantity of Money, 1969, p.73.
11. M. Friedman, "A Monetary Theory of Nominal Income", Journal of Political Economy, 1971, p.323. "The difference between the two theories is in the missing equation - the quantity theory adds an equation stating that real income is determined outside the system (the assumption of 'full employment') the income-expenditure theory adds an equation stating that the price level is determined outside the system (the assumption of price or wage rigidity)." Ibid., p.323. Brunner however characterizes Friedman's Monetarism as 'weak' and argues that, "Friedman's acceptance of the IS-LM framework and [his] view of the transmission mechanism brings him into general agreement with the neo-Keynesians . . . We regard as most important that . . . [Friedman's] theories do not generate principle monetarist conclusions about the role of money and the variability of monetary policy", K. Brunner and A.H. Meltzer, "Friedman's Monetary Theory", Journal of Political Economy, 1972, p.846 and p.837. However, from what seems from the controversy literature, the conflict does not run along the lines implied by Brunner's 'strong' position, but rather by Friedman's 'weak' one. Although adherents of the 'strong' Monetarism have provided empirical evidence supporting their thesis, both in respect to the 'weak' and fiscalist positions, nonetheless conflict has stemmed mainly from encounters between the 'weak', according to Brunner, Monetarism and Fiscalism.
12. On the IS-LM model's role in the controversy see Teigen, "A Critical Look . . .", op.cit., pp.11-15.
13. See H.G. Johnson, Macroeconomics and Monetary Theory, 1971, p.123. For a discussion of the debate on the issue of the "missing equation" see the articles written by Friedman, Harrod, Tobin, Brunner and Meltzer, and Patinkin, in the Journal of Political Economy, 1972, pp.886-950.
14. For instance Friedman talking of Keynes' model contends that in it Keynes "assumed that, at least for changes in aggregate demand, quantity was the variable that adjusted rapidly, while price was the variable that adjusted slowly, at least in a downward direction. Keynes embodied this assumption in his formal model by expressing all variables in wage units, so that his formal analysis . . . dealt with 'real' magnitudes, not 'nominal' magnitudes", "A Theoretical Framework . . .", op.cit., p.18. However, Patinkin in pointing out the monetarist oversimplification of the Keynesian model comments that, "to express all variables in wage units - that is, to deflate nominal quantities by the wage rate - is surely not to assume that this unit is constant . . . Similarly to express a model in 'real magnitudes' does not mean to assume that wages and prices are rigid or" (cont.)

## FOOTNOTES (cont.)

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14. (cont.) exogenously determined. It is, instead simply to assume that there is no money illusion in the system", "Friedman On the Quantity Theory and Keynesian Economics", op.cit., pp.895-6. Friedman however answers that these kind of arguments are only "qualifications made . . . when confronted with the criticism, which does not alter the price constancy assumption", "A Theoretical Framework . . .", op.cit., p.20.
15. The full employment assumption implicit in the Walrasian market has been relaxed in a further article by Friedman ("A Monetary Theory of Nominal Income", op.cit.) in which he employs a combination of the two approaches. For criticism see Teigen, "A Critical Look . . .", op.cit., p.12, J. Tobin, "Friedman's Monetary Theory", Journal of Political Economy, 1972, pp.851-63, and Brunner and Meltzer, ibid., p.837.
16. In analysing the 'naive' Keynesian model Mundell concludes that, "the . . . analysis is unsophisticated because it does not explicitly account for supply conditions and because the price level and wage are assumed to be constant" (my emphasis), R. Mundell, "An Exposition of Some Subtleties in the Keynesian System", in Johnson and Kamerschen, "Macroeconomics", op.cit., p.34. See also F.S. Broome, Macroeconomics, 1970, p.125, and W. Smith, "A Graphical Exposition of the Complete Keynesian System", in Johnson and Kamerschen, ibid., pp.19-26.
17. Tobin, ibid., p.852.
18. Friedman, "A Theoretical Framework . . .", op.cit., p.20.
19. Teigen, "A Critical Look . . .", op.cit., p.15.
20. Teigen says that, "rather than assuming that prices are fixed as a means of making the simple static model determinate, modern Keynesians introduce an aggregate labour market and production function in the analysis . . . the standard static pcomplete Keynesian system' is . . . one in which the general price level is one of the variables determined by the interaction of the system . . .", "A Critical Look . . .", op.cit., p.15. However, even the wage-rigidity assumption, according to Keynesians may be relaxed with no fundamental alteration of the 'sophisticated' Keynesian system. See for example Mundell who in fact examines the properties of the system with the assumption that "prices and wages are both flexible", ibid., p.32.
21. Patinkin, "Friedman On the Quantity Theory . . .", op.cit., p.900.

## FOOTNOTES (cont.)

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22. Leijonhufvud, "Keynes and the Keynesians . . .", op.cit., p.212. However, Coddington argues that the difference between, what he calls "reductionists" and "Fundamentalist Keynesians" is deeper and lies in Keynes' effort to bring uncertainty and vagueness into economic theory, to bring, as Coddington says, "the liberation from equilibrium theorising, as an escape from the restrictions that it imposes on our thinking", "Keynesian Economics . . .", op.cit., p.1261 and p.1260. Also, J. Eatwell has recently argued that the earlier works by Keynes involve assumptions that are in direct confrontation with equilibrium theory (seminar given in the Department of Economics at the University of Edinburgh). Whatever the difference between Keynes and the 'Classical' equilibrium school, it is sufficient to argue from the point of view of this chapter that Keynesianism, in whichever form, took a step towards the inclusion of more 'realistic' assumptions about the workings of the economy.
23. For further evidence of the price endogeneity assumption in Keynesian thinking, see R.S. Halbrook, "The Interest Rate, The Price Level and Aggregate Output", in W.L. Smith and R.L. Teigen, Readings in Money, National Income and Stabilization Policy, 1970, pp.43-65. Also F. Modigliani, "The Monetary Mechanism and Its Interactions with Real Phenomena", Review of Economics and Statistics, 1963, pp.79-107, and Teigen, ibid., p.15.
24. L.C. Andersen and K.N. Carlson, "St. Louis Model Revisited", International Economic Review, 1974, pp.309-310.
25. A. Ando, "Some Aspects of Stabilization Policies, The Monetarist Controversy, and the MPS Model", International Economic Review, 1974, pp.568-569.
26. "[F]iscalists . . . [do] not distinguish between market rates and real rates . . . [they] calibrate monetary action in terms of market interest rates and frequently identify a constant monetary policy with stable interest rates", D.I. Fand, "Monetarism and Fiscalism", Banca Nazionale del Lavoro Quarterly Review, 1970, p.284 and p.290.
27. "Monetarists calibrate monetary action in terms of the growth rates of the monetary aggregates", Fand, ibid., p.290.
28. Op.cit., p.374.
29. Teigen, "A Critical Look . . .", op.cit., p.19.
30. For a mathematical proof of the above see Teigen, ibid., Appendix. Also for a detailed exposition of the argument see ibid., pp.16-7.
31. Fand, ibid., p.277.

## FOOTNOTES (cont.)

## PART 11: CHAPTER 4:

32. For a discussion of the role of the price level in the Keynesian world see J. Hicks, The Crisis in Keynesian Economics 1974, where he speaks in terms of "flexprice" and "fixprice" markets, pp.23-30.
33. Mundell, "An Exposition . . .", op.cit., p.32.
34. Bank of England, "The Importance of Money", in H.G. Johnson, Readings in British Monetary Economics, 1972, p.16.
35. P.A. Samuelson, "Reflections on the Merits and Demerits of Monetarism", in Diamond, "Issues in Fiscal and Monetary Policy", op.cit., p.10.
36. D. Laidler, "The Influence of Money on Economic Activity - A Survey of Some Current Problems", in G. Clayton, J.C. Gilbert and R. Sedwick, Monetary Theory and Monetary Policy in the 1970s, 1971, p.108 and footnote 41.
37. Hence the two articles written by Friedman, ("A Theoretical Framework . . ." and "A Monetary Theory . . .", op.cit.) in which he provided an answer to the criticism that Monetarism did not provide a theoretical explanation of the effects of money on income.
38. K. Brunner, "Comment", on L.C. Andersen's "The State of the Monetarist Debate", FRB of St. Louis Review, September 1973, p.9.
39. Friedman, "A Theoretical Framework . . .", op.cit., p.28. See also Brunner, "The Role of Money . . .", op.cit., p.199, and Fisher and Sheppard, "Interrelationships between Real and Monetary Variables: Some Evidence from Recent U.S. Empirical Studies", in Johnson and Nobay, Issues in Monetary Economics, op.cit., p.180.
40. Hicks, "The Crisis in Keynesian Economics", op.cit., p.37.
41. Teigen, "A Critical Look . . .", op.cit., p.18. For a discussion of the development of the Keynesian theory of portfolio see J. Tobin, "An Essay on Principles of Debt Management", in "Stabilization Policies", Commission on Money and Credit, Fiscal and Debt Management Policies, 1963, pp.143-218, and for a discussion of the explicit generalization of the portfolio in large econometric models see F. De Leeuw and M. Gramlich, "The Channels of Monetary Policy", Federal Reserve Bulletin, 1969, pp.472-491. Also in an article written by members of the Research Staff of the Bank of England, the authors conclude that, "Expansionary monetary policy . . . will cause rates of return on a very wide range of assets, including stocks of all real goods, to be higher, at the margin, than the return available on money (cont.)

## FOOTNOTES (cont.)

## PART II: CHAPTER 4:

41. (cont.) balances and other financial assets. In this general sense monetary policy is transmitted to expenditure decisions via interest rates", "The Importance of Money", op.cit., p.34. Also in the transmission mechanism as it is expounded in the FRB-MIT model, monetary impulses are transmitted through "first, portfolio effects due to changes in relative yields on different assets; second, wealth effects brought about by changes in the net worth of consumers; third, credit rationing effects which are due in some degree to institutional peculiarities of the U.S. financial system", R.L. Teigen, "The Keynesian-Monetarist Debate in the U.S.: A Summary and Evaluation", in the Statsokonomisk Tidsskrift, 1970, p.12. A general portfolio theory, according to fiscalists, has always been explicit in Keynesian models, in which monetary and real sectors interactions have been included. In fact the FRB-MIT contains six different channels (see ibid., pp.13-4).
42. Friedman, "A Theoretical Framework . . .", op.cit., p.28.
43. ". . . there is no fundamental 'flow in the system'" says Friedman "that makes unemployment the natural outcome of a fully operative mechanism", ibid., p.17, (except what he calls the 'natural rate of unemployment', see his Inflation and Unemployment: The New Dimension of Politics, 1976).
44. Teigen, "A Critical Look . . .", op.cit., p.21.
45. Ibid., p.21.
46. "Once the semantic issues are put aside . . . the Tobin, Brunner-Meltzer, Friedman approaches to the relative price channels of monetary influence are quite similar", R.W. Spencer, "Channels of Monetary Influence: A Survey", FRB of St. Louis Review, 1974, p.14.
47. Friedman, "Theoretical Framework . . .", op.cit., p.27.
48. "There is no long-run effect of a monetary shock on the factors which influence the trend growth of output. It is generally accepted that the trend growth of money . . . and money demand influence the trend rate of price increase." L.C. Andersen and D.C. Karnosky, "The Appropriate Time Frame for Controlling Monetary Aggregates: The St. Louis evidence", in Controlling Monetary Aggregates, II, 1972, p.149.
49. Friedman, ibid., p.27. Also Brunner says that, "The existence of a mutual interaction over the shorter-run between money and economic activity, however, must be fully acknowledged", "The Role of Money . . .", op.cit., p.201.

## FOOTNOTES (cont.)

## PART II: CHAPTER 4:

50. Friedman, "Money and Economic Development", op.cit., p.3.
51. Patinkin, "Friedman on the Quantity Theory . . .", op.cit., p.886. Also Spencer observes that, "To a large extent, the differences in the [opposite] views are due not so much to contradictory theories, but rather shades of emphasis among similar approaches", ibid., p.13.
52. In making the distinction between theories and 'programs' Coddington reaches the same conclusion when he says that since schools "are alternative programs for theorising, rather than alternative theories, they revolve around matters of emphasis", "Keynesian Economics . . .", op.cit., p.1265.
53. "The strength of each type of policy is affected by the sizes of both 'real' and 'monetary' parameters and elasticities, and there are conditions under which each policy instrument will be impotent", Teigen, "The Keynesian-Monetarist Debate . . .", op.cit., p.26.
54. "Money does not alone matter, and the debate goes on as to whether in some sense money matters most or doesn't", P.A. Samuelson, "Money, Interest Rates and Economic Activity: Their Interrelationship in a Market Economy", in Johnson and Kamerschen, Macroeconomics, op.cit., p.145.
55. See Coddington, "Rethinking . . .", op.cit., p.435, and N. Bruce, "The IS-LM Model of Macroeconomic Equilibrium and the Monetarist Controversy", Journal of Political Economy, 1977, esp. p.1049.
56. B. Sprinkel, "The Effects of Monetary Change", in Johnson and Kamerschen, Macroeconomics, op.cit., p.173.

CHAPTER 5

E M P I R I C A L   E V I D E N C E   A N D  
T H E   R E S O L U T I O N   O F  
T H E   M O N E T A R Y   C O N T R O V E R S Y

"Every man, who has ever reasoned in this subject,  
has always proved his theory, whatever it was, by  
facts and calculations."

D. Hume, "Of the Balance of Trade"

"[D]ifferent economists are going to look at the same  
body of data with equal confidence that it supports  
their very different theoretical schemes. No matter  
what the economic facts are . . . W. Heller and  
M. Friedman are both going to tell . . . that they  
are right . . . What are we to believe?"

L. Klein<sup>1</sup>

## CHAPTER 5:

EMPIRICAL EVIDENCE AND THE  
RESOLUTION OF THE MONETARY  
CONTROVERSY

## A. INTRODUCTION

The purpose of this chapter is to review the major pieces of empirical evidence that have been brought forth in order to resolve the controversy. If the hypothesis, according to Positive economics, that empirical evidence represents reality is valid, then the resolution of the conflict should be an easy task. The choice between rival hypotheses should be dictated by an appeal to empirical reality. However, as we shall see, although empirical tests of the alternative hypotheses have been carried out, neither group has been ready to accept the results. There has always been some kind of qualification or criticism, either methodological or theoretical, that rendered the empirical results, according to each faction, inapplicable<sup>2</sup>.

The scope of this review will be restricted to empirical evidence that has stemmed mainly from major monetarist and fiscalist tests. Firstly, I shall review the monetarist tests, i.e. the money-income relation evidence, the Friedman-Meiselman tests, the Andersen-Jordan tests, and the counter-offensives by Fiscalists, comprising the so-called single-equation tests; and secondly, the fiscalist tests, i.e. structural models and the counter-offensives by Monetarists, comprising the large-scale tests.

## B. FORMS OF THE CONTROVERSY

Since the appearance of the first empirical attacks that the Monetarists levelled against the Fiscalists, the criticism and counter-attacks that have emerged shaped the debate in numerous forms that reflected the improvements in techniques and availability of data. In fact the old issues in the debate have remained the same and the only thing that did change was the determination of empirical economic reality. The ad hoc evidence presented before the emergence of the Friedman-Schwartz and Friedman-Meiselman attack was seen as inadequate and inconclusive. The methods used by the Radcliffe Report to support, say, the interest inelasticity of business expenditures was to listen "to opinion rather than analyse . . . data . . . No tests were conducted and no data were surveyed to examine the validity of this contention"<sup>3</sup>. Despite this 'unpositivism' however there emerged a tremendous reaction that spurred the expansion, improvement and widespread use of empirical testing and investigation. In consequence the controversy took a turn away from the mere verbal or purely theoretical conjecturing - the Radcliffe Report, for instance, rejected the quantity theory of money on the "tacit knowledge" that first, monetary policy was "incidental to interest rate policy" and second, that "there is [no] limit to the velocity of circulation"<sup>4</sup> - to the 'firm' and 'conclusive' grounds of empirical testing.

Before examining each set of empirical results that were brought forth it is appropriate to list the issues and empirical hypotheses that were put to test. The first one, associated with

Friedman and Schwartz, was concerned with the question whether money does or does not matter in the determination of aggregate economic activity. The second issue, associated with Friedman and Meiselman, was concerned with the comparison of the monetary and fiscal multipliers in an attempt to establish whether the money supply or autonomous expenditure mattered more. The third issue, associated mainly with Andersen and Jordan, was partly an elaboration and perhaps an improvement of the previous issue, and related to the relative efficacy of fiscal and monetary policy. These issues were also tested within the large-scale model framework. There were also other issues related to the transmission mechanism, or the demand for money function, its stability and the role of the interest rate in it<sup>5</sup> (these latter issues are, however, outside the scope of this chapter).

The way and the method with which the above issues were studied depended on which side of the 'ring' one happened to be. In the one corner there were the Monetarists propounding the simple reduced-form, single-equation tests and in the other there were the Fiscalists supporting the use of large-scale structural econometric models as a means to extract evidence<sup>6</sup>. However, although both factions are characterized by different methods, both of them undertook to carry out tests that were based on the method used by the opposite group. For instance De Leuve and Kalchbrenner tested Andersen and Jordan's model with a different specification, and Andersen tested the merits of Fiscalism and Monetarism by constructing a model along the lines of the large-scale models, albeit in a reduced form.

In respect to the initial distinction between the tests, however, the choice of method made by the Monetarists was justified by the fact that for them there were two separate empirical hypotheses to be tested: whether money matters or it does not matter - hence the estimation of the correlation between changes in the money stock and the level of money income - and whether fiscal or monetary policy was more important in stabilizing the economy. Single-equation tests were simple and if the exogeneity of the independent variables was statistically determined then a strict comparison of the magnitudes of the coefficients would presumably decide the truth or falsity of the hypothesis in question. One vague point in this procedure was that it was never made clear whether the testing of the equations was also a testing of the implications of each theory. For instance in the Andersen-Jordan tests, although the equations were sometimes called 'reduced forms' the structural model was not, allegedly, made explicit. The term 'reduced form' was perhaps meant to imply a relationship between exogenous and endogenous variables, that is to say between autonomous and induced-by-the-system variables. However, although the proper econometric procedure implies a relation similar to the latter, it differs in the respect that the reduced forms may be expanded to the full structural specifications and vice versa<sup>7</sup>. If this is true then, according to Fiscalists, the Andersen-Jordan test are not implications of any particular theory, but only independent empirical hypotheses. Nonetheless the results of these tests were used by Monetarists as evidence for their support of their theory<sup>8</sup>, and this generated a great deal of criticism not only from Fiscalists but also from supporters of the 'stronger' version of Monetarism<sup>9</sup>.

On the other side of the debate the Fiscalists justified their method by pointing out the multidimensionality of the macroeconomic system and the need for allocative detail. At least as far as the sophisticated version of Fiscalism is concerned, there is not any one particular factor that should be assigned the causal role. Fiscalists maintain that since one has to take account for all the channels through which the effects are transmitted then one needs a multisectoral, large-scale econometric model. This should encompass the most important and relevant features of the economic mechanism, from which the estimation of the parameters, having taken into account the totality of the system, will serve as evidence for the discrimination of the two different theories. According to Fiscalists, then, it is within this methodological framework that a Keynesian hypothesis may be appropriately tested. However, as we shall see, in addition to this type of evidence most Fiscalists tried to fight the Monetarists on the same battleground, using single-equation tests.

Having seen the forms around which the testing of the alternative hypotheses revolved, I now turn to examine the resulting empirical evidence and see whether it was decisive in resolving the debate.

#### C. SINGLE-EQUATION TESTS

##### a. THE MONEY-INCOME RELATION

In a book called "A Monetary History of the U.S. 1867-1960"<sup>10</sup> Friedman and Schwartz presented evidence that was meant to support the

hypothesis that money does matter. They compared income changes to the rate of change of the money stock per unit of time, and found that practically in all periods examined, the money supply changes preceded changes in income. According to this investigation empirical evidence seemed to have changed prior perceptions of experience. Even for the period of the late '20s and early '30s it was revealed that money, in opposition to what was generally believed, did matter. All this implied, for the Monetarists, that money had a controlling influence on changes in income. In fact, according to this evidence, it was not true any more what Keynesians had believed up to then, namely that low interest rates, liquidity traps, or the instability of velocity could have a pronounced effect in deterring money from having a systematic relation with income<sup>11</sup>.

However, due to the variability of the time lag from cycle to cycle it was admitted that money could not be used as a 'fine-tuning' instrument, but rather that it should be allowed to grow at a constant rate of, say, three to five per cent per year in order to correspond to the needs of the growing economy and yet not cause any inflation. In essence, this finding justified the monetarist ontological assumption of the flexibility of the market and the consequent non-interventionist stance. In other words, given the 'freedom' and flexibility of the market any 'fine-tuning' would upset the balance. However, since any machine needs some oiling, so the economy needs a 'constant rate' of money supply growth in order to operate smoothly. Effectively then, this evidence was directly and indirectly used, first to assert the significance and influence of money in the economy,

and the 'laissez-faire' stance, and second to discredit the fiscalist propositions, stressing the importance of interest rates as instruments of monetary policy, and the consequent interventionist stance.

The criticisms and reactions that this evidence generated were strong and numerous. Kaldor, for instance, emphasized the fact that there was nothing inherent in the money-income statistical correlation that could show the direction of - if any - causation. Was money influencing income or the other way around? The fact that money supply changes come chronologically first does not mean that they also determine changes in the level of income causally. In fact, Kaldor argues, "There is every reason for supposing . . . that the rise in the 'money supply' should precede the rise in income - irrespective of whether the money-increase was a cause or effect"<sup>12</sup>. Moreover, the preceding in time of money does not entail that there is any exogeneity in its variations; on the contrary it "may merely be a reflection of the operation of the built-in fiscal stabilizer"<sup>13</sup> (although the application of lag structures could alleviate the problem of the statistical endogeneity of money, it could not, however, alleviate the problem of causation).

Other critics also responded in a similar fashion. One of them, Samuelson, not only dismissed this evidence as one that rejects Keynesian theory, but also maintained that "the evidence of timing and turning-points . . . is consistent with many different theories, and also is not a powerful test . . ."<sup>14</sup>. Also Davis criticized the Friedman-Schwartz findings by saying that,

The Friedman-type cyclical analysis has never seemed to me to grapple adequately with the implications of the existence of a strong reverse effect running from business to money. The existence of such a reverse effect undercuts seriously the evidential value of the historical timing pattern of business and monetary cycles in establishing the causative role of money.<sup>15</sup>

Finally, Tobin and Swan after having re-estimated the Friedman-Schwartz model give the final blow by saying that, "Contrary, perhaps, to much popular belief, the evidence does not support that there is a simple direct relationship of income to money"<sup>16</sup>. In addition Tobin and Swan estimated the predictive capacity of the model and found that the predictions generated diverged significantly from the actual path<sup>17</sup>. Moreover, the conclusion, implied by the Friedman-Schwartz findings, to the effect that interest rates are insignificant as policy instruments, has also been contested by Tobin and Swan's findings, who add that, "While Friedman has doubted the empirical significance of interest rates, other than expected changes in the value of money, on the demand for money, other researchers have found evidence of such influence"<sup>18</sup>. The consequence of the evidence that interest rates are significant is that the money-income relation is weakened. Once the significance is asserted other influences, besides money, play an important role in the determination of economic activity. As Tobin and Swan confirm, "Given such sensitivity, (of money demand to interest rates) short-run fluctuations in income can have non-monetary as well as monetary causes"<sup>19</sup>.

However, despite this fiscalist torrent of criticism and counter-attacks, Monetarists still hang on the money-income correlation and still claim that this evidence constitutes one of "the more

important facts we have obtained from recent experiences"<sup>20</sup>, and that it is "the most firmly established empirical association in all economics"<sup>21</sup>. And yet Fiscalists would respond that this "fact" was a mere "gross association"<sup>22</sup> or that "timing evidence" - leads, lags and so on - is no evidence about causation whatsoever"<sup>23</sup>.

But where do Monetarists base their belief in this correlation? Firstly, they claim that there is, over a long period, quite a close association between money and income, and secondly, in almost all phases of the cycle money precedes income, though admittedly with a variable lag. These two evidential points then, imply, according to the Monetarists, controlling influence. Controlling influence, however, implies exogeneity and, according to Fiscalists, this is not true as far as money is concerned. The problem then centers around the exogeneity of money. While Monetarists claim that, "no spurious statistical element whatsoever is present . . . The income series in each country is statistically independent of the money series"<sup>24</sup>, Fiscalists, on the other hand, base their claim that money is an endogenous variable on the following reasons: (a) they claim that one cannot argue that money is independent of other factors not included in the system, and (b) they claim that one cannot know that the association runs from money to income and not vice versa. Thus, according to Fiscalists, although money is an important element within the total picture of economic policy, which is what, according to them, was shown by the correlation, nonetheless it cannot be assigned an exclusive causal role. Moreover, Fiscalists argue that the recognition that money is important does not mean that fiscal policy

is unimportant. As Samuelson emphasizes,

Most of the evidence is conclusive with respect to a . . . view that money doesn't matter, but as to the view that money matters and that fiscal policy . . . does not have a systematic influence, there is very little of the . . . evidence that is germane to that.<sup>25</sup>

In addition there is the argument of the factors outside the relation, which nonetheless influence it. For example, built-in stabilizers, movements in interest rates, counter-cyclical government policy, are some of the factors liable to produce evidence showing money systematically to precede income without this implying or indicating any causal relation<sup>26</sup>. "The substantive issue therefore", as Davis says, "remains whether or not money is the dominating exogenous influence on the cyclical course of business"<sup>27</sup>.

Thus, on the one side Monetarists believe in the exogeneity of money on statistical grounds, i.e. the money series being significantly independent of the income series, while on the other side Fiscalists believe in the endogeneity of money on economic grounds. Besides some assurance that the correlation is not spurious, Monetarists have not offered a satisfactory answer to the fiscalist accusation of economic endogeneity. They have rather insisted on the importance of this evidence as proving that money matters more than budget manipulation, and referring to the fiscalist criticisms of the necessity of determining explicitly the causation of the relation as mere "qualifications"<sup>28</sup>.

Despite, therefore the 'hardness' of the evidence brought forth to support the monetarist thesis, it was not accepted by the

Fiscalists. Inconclusiveness was as much true of the Friedman-Schwartz evidence as of qualitative judgements of the Radcliffe Report type. In order to counter-attack the above criticism and bring 'harder' evidence that would prove the relative merit of Monetarism, the Monetarists turned to a different type of evidence produced from tests that compared the relative empirical strength of each hypothesis.

b. THE FRIEDMAN-MEISELMAN TESTS

After the Monetarists had established a correlation between money and income, they went a step further to compare and test the relative performance of the quantity theory against the fiscalist autonomous expenditure model<sup>29</sup>. It was believed that if the two theories could be separated into two distinct models then the estimation of these models would provide results capable of resolving the controversy<sup>30</sup>.

The rationale behind this test was to determine which variable, representing each theory, had the most systematic, quick and predictable effect on the level of consumption. Consumption was chosen instead of income because given that nominal investment is a part of income then the use of nominal income would produce spurious results in favour of the fiscalist position. The variables to represent each theory were: autonomous expenditure, including investment, exports-imports, government expenditure and tax collections for the fiscalist side, and some definition of the money supply for the monetarist side. The models were presented as two single regression equations, the estimated coefficients of which represented the multipliers of each theory. The two regressions were run over a period of sixty years, finding that the performance of the

money supply was superior to that of autonomous expenditure, performance being defined as consistency of the parameter estimates and in terms of the magnitudes of the correlation coefficients. In other words results from this test were interpreted as indicating the relative strength of money over autonomous expenditure in explaining consumption, and by implication income. For all periods, besides the Depression years, Friedman and Meiselman (hereafter called F-M) found that "the stock of money is unquestionably far more critical in interpreting movements in income than is autonomous expenditures"<sup>31</sup>.

The criticisms that were stimulated by this test came mainly from Ando and Modigliani, De Prano and Moyer and Hester<sup>32</sup>. The three independent critiques agreed on one fundamental point: that F-M's empirical results were very much determined by the particular conception and definition used to represent the fiscalist position. Hester argued, for instance, that "Their statistical comparisons are extremely sensitive to how the autonomous expenditure theory is represented"<sup>33</sup>. Or Ando and Modigliani claimed,

that the relatively low correlation coefficients between C [consumption] and A [autonomous expenditures] obtained by FM are due to several very serious misrepresentations of the Keynesian theory and its observable implications.<sup>34</sup>

Finally, De Prano and Mayer contended that,

FM's finding that predictions based on the autonomous-expenditure equation were inferior to those based on the money equation was due to a particular definition of autonomous expenditures.<sup>35</sup>

Apart from the definitional problems, the critics contested the reliability of the F-M's tests and pointed out their various other shortcomings. For example, misspecification of the consumption function, inclusion of war years in the period examined, the economic as distinct from the statistical exogeneity of the independent variables<sup>36</sup>, or the use of levels instead of first differences<sup>37</sup>, were some of the defects on the basis of which Fiscalists did not accept the F-M results. Moreover, the often repeated fiscalist criticism to the effect that the discrimination of the competing hypotheses cannot be achieved on the basis of simple-equation tests, was brought forth. This argument is based on the belief that due to the complexity of macroeconomic phenomena and the manifold interrelations that need to be represented with a structural and complex model, single-equation tests cannot distinguish between the effects of money and autonomous expenditures. For instance, Ando and Modigliani argued that,

the results reported by FM contain very little, if any, information about the empirical usefulness of that model. No really adequate test of this model appears feasible within the constraint of a single equation.<sup>38</sup>

Unless a more realistic overview of the economy is undertaken, the single-equation tests put a severe constraint, "an artificial straight-jacket"<sup>39</sup>, on the tested models. As Poole and Kornblith reported,

neither the simple Keynesian nor the simple quantity theory models provide an adequate understanding of business cycle fluctuations . . . There simply is not much empirical content in the single-equation approach as employed in the studies examined here.<sup>41</sup>

The results, therefore, were not accepted on the basis of their simplicity<sup>41</sup>.

In addition to these criticisms Fiscalists provided empirical evidence produced from tests that were equivalent to the F-M ones. By using modified definitions, especially of the autonomous expenditures model, excluding war-time periods and removing "some of the oversimplifications and misspecifications"<sup>42</sup> used in the F-M tests, Fiscalists tested the models and found that the impact of autonomous expenditures on consumption was greatly understated in the F-M tests. They claimed to have found that although money was important in explaining consumption nevertheless autonomous expenditures, contrary to what F-M found, was on the average, or perhaps more, important. This result, however, was constrained by the assumption that the tests could discriminate between the two models inasmuch as the simple-equation, "straight-jacket", procedure was accepted. More generally, they claimed that one should not rely on this procedure as it would produce artificial results<sup>43</sup>. Hester, for example, "'using' a more conventional representation of the autonomous expenditures theory . . . and some of their data" found that "little empirical evidence . . . favors the quantity theory"<sup>44</sup>. Also by using alternative definitions of autonomous expenditures he found that "correlations . . . exceed .90 in non-World War II years"<sup>45</sup>. In essence, according to the empirical evidence that he adduced, "Friedman and Meiselman's conclusion that a simple quantity theory performs better than a popular autonomous expenditure theory in correlation tests is false"<sup>46</sup>. In addition Hester computed first differences instead of levels and found that irrespective of the measure to represent autonomous expenditures the fiscalist model "outperforms the monetary model . . . in nonwar years"<sup>47</sup>. Also De Prano and Mayer by respecifying slightly the models

tested found that,

although money is not completely exogenous, we are in general agreement with FM's positive conclusion that money is important. We reject, however, their negative conclusion that autonomous expenditures are not important.<sup>48</sup>

Finally, Ando and Modigliani after having redefined the fiscalist position concluded that their, "tests are fully consistent with the Keynesian view that both mechanisms play a role" and that, "the impact effect of autonomous expenditures on current expenditure is shown to remain quite substantially"<sup>49</sup>. Also concerning the impact effect of money on expenditure, it was found that after modifying certain assumptions of exogeneity in relation to money, it was much smaller than was found in the F-M tests<sup>50</sup>. In short, all critics of the F-M paper found empirical results significantly contradicting the results produced by F-M.

This counter-evidence, however, did not seem to convince F-M who reacted by replying that,

None of the calculations made by our critics . . . is correct because they omit components of income for the income-expenditure calculations, set the two theories different tasks, or use lengthy periods combining two different subperiods.<sup>51</sup>

Concerning definitions, F-M claimed that their criteria for choosing the particular ones used in their test were strictly empirical, whereas the Fiscalists' were largely intuitive and a priori<sup>52</sup>. "[I]n our opinion" F-M argued, "our critics have neither established that our measure [of autonomous expenditures] biased the results nor demonstrated

that their alternative measures are more defensible in terms of relevant criteria"<sup>53</sup>. Concerning the exclusion of wartime periods they responded that it "is a debating point pure and simple . . . It has no relevance to our papers"<sup>54</sup>. However, F-M did not confine their response to theoretical counter-critiques, but they also provided more empirical evidence using some of the alternative definitions offered by the Fiscalists. F-M conclude,

We have made some of the correct calculations for one of the alternative concepts of autonomous expenditures . . . Though less clear-cut, the results are in the same direction as those from our original calculations.<sup>55</sup> (my emphasis)

Even with these new calculations F-M did not succeed in persuading the Fiscalists of the superiority of the monetarist position. Fiscalists stayed incontravertibly in their initial position. For instance, Hester exclaimed that,

someone might have construed the Friedman-Meiselman paper as saying something about the relative usefulness of the autonomous expenditure and quantity theories. I have shown that their results are inconclusive.<sup>56</sup>

Or Ando and Modigliani replied that they,

cannot . . . accept what seems to be FM's main criticism of our procedure for testing the income-expenditure model . . . The specification of our model was chosen to fit, as closely as possible, the standard Keynesian formulation . . . their procedure using simple correlations . . . is no less heuristic than our appeal to common sense and informal observation, although it appears so much more formidable and objective.<sup>57</sup>

Finally, additional evidence against the monetarist position came from applications of the F-M test to British data by Barret and Walters<sup>58</sup>. Regarding the monetarist position their results differed

quite substantially from the F-M ones, and regarding the fiscalist position, with a slight change of specification, the autonomous-expenditures model seemed to perform better<sup>59</sup>. Although the obvious criticism against these results was that the different characteristics of the British economy accounted for the divergence of the results, a study made by Argy showed that the difference in the two economies were minimal relative to the application of the test and that therefore the results were comparable<sup>60</sup>.

In effect, then, one can safely conclude that the F-M empirical evidence failed to convince the Fiscalists of the relative prominence of the monetarist position. It would appear, from the above account, the definitional problem loomed quite large. Evidence and counter-evidence was produced with different specifications or modifications of the models tested. Even though the Monetarists argue that, "important substantive conclusions seldom hinge on which definition is used"<sup>61</sup>, and, "that the definitional problem is not an obstacle . . . [and] it has been shown that different definitions of money give consistent results . . ."<sup>62</sup>, and therefore brush away the problem, it is nevertheless true that different definitions have been applied and different results have been found. Pierce and Shaw, for instance, found that, "successive narrowing of the definition of A [autonomous expenditure] tends to raise the explanatory power of A from 38% to 80%"<sup>63</sup>. In fact Fiscalists insisted that the, "definition of autonomous-expenditure is open to a wide variety of interpretations and the results of tests using different definitions will lead, in general, to large variations"<sup>64</sup>, or that, "the use of

slightly different definitions will lead in general to different results"<sup>65</sup>. Thus the problem seems to revolve around the question of whether there could be one set of definitions acceptable to both sides of the debate unconditionally. However, if the problem is cast in these terms, then the definitional investigation requires theoretical insight that will determine the empirical scope. This, however, leaves the Positive economist with empirical facts that are theory laden and therefore, according to him/her, subjective and unacceptable. If on the other hand the definitions are determined strictly empirically, the question arises as to what validates the criteria with which the empirical truth and content of the definition of the variables is determined? Surely not theory, and surely not empirical evidence. In narrowing the problem to one of definition and conceptual determination of the variables, the task of the Positive economist, namely to empirically objectify theories, is undermined.

It is interesting to note in this context that the definitional problems facing the F-M empirical results is a manifestation of the methodological problems discussed in part I. The ambiguities in the definition of 'theoretical' or 'empirical' mentioned there allow sufficient leeway for alternative interpretations of what theory or fact should mean. Both Monetarists and Fiscalists in being inconsistent in this definition stumble on the obstacle of the impossibility of drawing a demarcation line between theory and fact. Thus, the methodological difficulties facing Predictionists and Assumptionists rear their ugly heads among Monetarists and Fiscalists. It seems that

perpetual modifications and respecifications produce contradicting empirical evidence perpetually.

In concluding, we see that although the F-M tests were perhaps significant in establishing the importance of money, a proposition not rejected by Fiscalists, it was, nevertheless, neither conclusive nor sufficient in being able to discriminate between the two theories. In short the objectivity, so much sought after, could not be found among either sides' empirical evidence. It seems that for various reasons neither did the Fiscalists ever accept the objectivity of the F-M results, nor did the Monetarists accept the counter-criticism. As Brainard and Cooper report 10 years after the F-M tests, "In retrospect, it appears clear that it is not possible to discriminate between competing macromodels simply on the basis of in-sample fits"<sup>66</sup>. Despite the inconclusiveness of this empirical evidence Friedman did rely on the F-M tests as evidence for the monetarist position<sup>67</sup>. Thus the stalemate between Monetarists and Fiscalists grew.

### c. THE ANDERSEN-JORDAN TESTS

In view of the controversial nature of the F-M evidence there arose some dissatisfaction among Monetarists as to the way the test was conducted. As a result new tests were devised and the specification was improved. The results elicited by the new tests were still in favour of the new quantity theory and against the fiscalist position. The credibility and forcefulness of Monetarism was enhanced by virtue of the refinement and sophistication of the single-equation tests.

Essentially, this sophistication was the result of discontent towards the testing of multipliers derived from models always assumed to be true and good approximations of reality<sup>68</sup>. The Andersen-Jordan tests<sup>69</sup> were considered improvements insofar as they offered direct tests of alternative economic hypotheses, in a 'purely' empirical manner, and not tests of the implications of any theory. Although they were sometimes called reduced-form tests there was nothing in the model to indicate the structure from which the reduced forms were derived<sup>70</sup>.

The tests were designed so as to determine directly the relative effectiveness of monetary versus fiscal policy. The multiple regressions calculated had as the independent variable measures of fiscal and monetary policy. The regressions were run over a period from I/1952 to II/1968. The refinements and improvements over the F-M tests, consisted mainly in the way in which each variable was measured. The variable chosen to represent fiscal policy consisted of changes in the full employment budget surplus and in the full employment federal expenditures and revenues. On the other side, monetary policy was represented by changes in the money supply and in the monetary base.

In measuring fiscal policy with the full employment budget, Andersen and Jordan managed to purge the relations of any income induced changes in the components of the budget, such as, for example, tax receipts. In addition the use of two measures of monetary policy, and especially the use of the monetary base, rendered the tests more immune

to the endogeneity-of-money criticism. The second type of improvement was in the use of distributed lags in which the shape of the lag was determined in the regression allowing only for the a priori imposition of the length of the lag<sup>71</sup>.

All in all the Andersen-Jordan tests were considered by both sides as a major step towards discriminating between the two theories. However, it must be emphasized that although Andersen and Jordan (A-J hereafter) claimed that, strictly speaking, the test was about two alternative empirical hypotheses, the results nevertheless were taken as a support of one theoretical position and the rejection of the other<sup>72</sup>.

The overall results of the tests were that fiscal policy was ineffective and that monetary policy was more potent. In fact the tax coefficients were presented as having perverse signs, i.e. an increase of taxes will increase G.N.P. In addition it was found that monetary policy acts more quickly than fiscal policy. According to A-J, "The response of economic activity to monetary actions compared with that of fiscal actions is (I) larger, (II) more predictable, and (III) faster"<sup>73</sup>. The results therefore seemed to support the monetarist position and for some Monetarists conclusively so. For Friedman in fact the success of the tests along with the rest of the evidence proved beyond any doubt the supremacy of the "monetary theory of nominal income"<sup>74</sup>.

However, despite the improvements in measurement and the assurance of the Monetarists, the Fiscalists did not accept the validity

of the results partly on some old familiar grounds and partly on the results of alternative tests. The old familiar arguments were those, first, of simplicity and, second, of endogeneity. Regarding the first criticism, the methodological question of single-equation tests was brought up again and the necessity of taking into account the interdependence of the whole system was emphasized. It was argued that if expenditure did not have any effect on income then this did not mean that fiscal policy was impotent, but rather that one of the other components of aggregate demand, unspecified behaviourally in the A-J test, must have changed perversely so as to offset the impact<sup>75</sup>. A greater knowledge of the complete structure of the system was therefore needed before anything could be said about the relative effectiveness of each policy. Regarding the second criticism, Fiscalists argued in terms of income induced money changes and of the definition of money. It was alleged that both definitions of money used in the tests were not exogenous and that adjustments were needed for the results of the tests to be valid<sup>76</sup>. It was also contended that the lags in the tests were unrealistic and did not conform to the common experience evidenced up to then<sup>77</sup>. It was also argued that the results depended upon the period chosen. Artis and Nobay applied similar tests to British data from a different period, 1958-1967, and found quite dissimilar results<sup>78</sup>. Also Walters applied the tests to 1955-1956 period data and found quite dissimilar results, at least as far as the money supply is concerned<sup>79</sup>. In addition Davis by splitting the period into two parts found that, for the earlier part at least, monetary policy performed quite poorly<sup>80</sup>.

However, the major criticism came from De Leeuw and Kalchbrenner (D-K hereafter) who ran the same regression on the same data only with a somewhat different specification of the policy variables<sup>81</sup>. As far as the specification of fiscal policy was concerned D-K argued that the exogeneity claimed by Andersen and Jordan for the fiscal variable was interpreted as an exogeneity in terms of the independence from the influence of policy makers<sup>82</sup>. However, they argued, a different interpretation of exogeneity, namely that of the independence of the variable from current endogenous variables, was not taken into account, with the result of bias in the findings. As one example of this misspecification, they claimed that although the full employment budget is independent of actual oscillations, nevertheless it does not take inflation into account. An adjustment to allow for price expectations was therefore considered necessary<sup>83</sup>. In addition D-K felt that the definition of monetary policy did not conform fully to the economic exogeneity condition. Component parts of the monetary base<sup>84</sup>, such as currency held by the public, were not independent of movements in endogenous variables representing economic activity. So they adjusted the monetary base to exclude currency. The results found after testing the respecified equations were quite dissimilar to the ones reported by A-J. As D-K report,

- (i) although the monetary policy variable remains the predominant influence in terms of t-ratios, the monetary multiplier decreases in size, and (ii) although the two fiscal variables remain insignificant statistically, the coefficients of the expenditures and receipts variables have the expected sign.<sup>85</sup>

Furthermore the quick action of monetary policy, shown in the A-J tests, also was proved to be much slower<sup>86</sup>. Thus D-K's results seemed to contradict A-J's. As D-K conclude,

We feel these results cast serious doubt on the Andersen-Jordan conclusions about fiscal policy. With alternative and highly plausible measures of Federal receipts and the monetary base, fiscal policy appears to exert a significant influence on GNP in the expected direction. Monetary policy also appears to exert a powerful influence.<sup>87</sup>

In reply A-J argued, however, that the alternative specifications were not economically plausible. They also maintained that D-K confused the issue of the definition of the monetary base<sup>88</sup>. They also claimed that "this process (of peeling the monetary base) has no economic relevance within the context of the customary body of economic theory . . ."<sup>89</sup>. Despite criticisms and counter-evidence A-J rejected the fiscalist modifications and held on their initial results<sup>90</sup>.

In conclusion, we see that the A-J tests though they constituted improvements of the F-M tests, did not produce empirical evidence sufficiently conclusive so as to decide between the fiscalist and monetarist positions. Indeed Fiscalists were persuaded that money mattered, an issue undisputed by them, but they were far from being convinced that measures representing fiscal policy were less strong in determining GNP than measures representing Monetarism. We have seen that empirical evidence presented by either side was not accepted and was interpreted according to different perspectives.

Again here as in the F-M tests, definitions, measurement difficulties and theoretical problems prevented empirical testing from fulfilling the positivist criterion of empirical validation. Thus theory was an inseparable element of empirical observations. In effect different specifications, or definitions, gave different results<sup>91</sup>. As Brainard and Cooper say,

Given the fact that economic data do not distinguish between economic views, the plausibility of theoretical explanation will continue to bear heavy weight in distinguishing among alternative hypotheses as well as in guiding our exploration of the data.<sup>92</sup>

Not a very happy prospective for the Positive economist who needs theory-free empirical facts in order to choose between alternative theoretical schemata.

We have thus seen that the empirical evidence produced by single-equation tests, and brought forth by Monetarists, have not proved decisive in resolving the Monetary controversy. Perhaps empirical evidence produced by large-scale complex models, brought forth by Fiscalists, might do better. The following section will briefly review the evidence and examine this possibility.

#### D. EVIDENCE FROM LARGE-SCALE ECONOMETRIC MODELS

Up to now I have presented evidence relating to the controversy from tests that are by and large monetarist. The assumption behind these tests was that single-equation models provide a simple and rigorous way for solving controversial issues. We have also seen

that the results based on these tests were not accepted by Fiscalists. This was due, firstly, to methodological and theoretical reasons and, secondly, to alternative evidence produced by similar tests conducted by Fiscalists and containing different specifications. It was claimed that findings derived from single-equation tests were not valid because they were either based on misspecifications of the equations, or they were models that did not explicitly specify the complete structure from which the behavioural relations were supposed to be derived<sup>93</sup>. From the point of view of the Fiscalists the controversial issues were far from being resolved with the evidence produced by single-equation tests.

In their stead Fiscalists offered results and empirical findings that were based on large-scale econometric models that specified the full structure of the economy. It was believed that only within this complex context could alternative policy formulations be appropriately tested. It is the purpose of this section to account for this type of evidence and see the extent to which it has succeeded in resolving the conflict and achieving convergence of the opposing views in the Monetary debate.

#### a. METHOD AND SCOPE OF LARGE-SCALE MODELS

The major objective of large-scale econometric models is to "mirror" complex reality in a systematic, quantitative, fashion. For the model builder there is no doubt that the economy is a set of interdependent and complex processes that need to be classified and determined quantitatively. A greater knowledge of the complexity of

the world implies a greater refinement and increase of the scale of the model, and therefore a greater predictive capacity. This is an attitude shared by most model builders. According to Fromm and Taubman,

For a system as complex as the modern economy, there are very few issues that can be examined in the simple, single-relationship *ceteris paribus* framework . . . What is needed is a quantitative simplification of the relations among economic variables which models the economy as system rather than as a set of unrelated random processes.<sup>94</sup>

To a certain extent this assumption of complex reality is also shared by the Monetarists. Friedman, for example, claims in his methodological writings that the world is indeed complex. But the way to look at it is not through a fully descriptive theory, but rather through simple, analytic, abstractions<sup>95</sup>. In fact, this rule constitutes an important tenet of Positivism in general, which sees phenomena in terms of observable, complex, processes the systematization of which defines the task of the scientist<sup>96</sup>. In this respect, i.e. in having a complex, unordered, world-view, both schools share the same positivist criterion. To a certain extent also they carry out this common perception of the world in a similar fashion, in that they both attempt to quantify observable phenomena. However, they differ in the following respect: for the Monetarists the world is far too complex to be modelled accurately. The way for them to study it is through simple conjectural hypotheses the tested predictions of which will generate knowledge about the world; hence the use of single-equation tests. "With regard to evidence", says Andersen,

"the testing of simple hypotheses is deemed to be more useful than the building of elaborate structural models"<sup>97</sup>.

For the Fiscalists, on the other hand, the more accurate and detailed a description of reality becomes the easier it is to capture the totality of the relations and to systematize one's understanding of unordered events and interdependencies. Although a cumbersome task for estimation, the continuous study of subsectors and the further disaggregation of the model provides the economist with greater insights as to the workings of the economy. It follows that to be able to make predictions, one needs to identify, as much as possible, all factors involved. According to Fiscalists a structural model that tends to approximate reality is more apt to make correct predictions than a less detailed one. For the Fiscalist model builder, all economic factors are relevant and should be included in a model. This is shown by the Fiscalists' insistence to disaggregate and try to make as many variables as possible endogenously determined. For Fiscalists causality lies nowhere and everywhere. For them the monetary sector is as important as the government in shaping economic policy<sup>98</sup>.

Another objective of the model builder is to use large-scale models to test alternative theories or policy proposals. The evidence produced from such tests are considered "systemic"<sup>99</sup>. That is, the tested coefficients are derived from a complete structure and their values are supposed to account for the workings of the entire system.

Fiscalists emphasize this in order to draw a contrast with single-equation tests, the coefficients of which are not considered reduced forms proper. For them the correct way is to attempt to reconstruct the full structure of the economy and specify a detailed financial subsector (the MPS model has 30 equations to account for financial events)<sup>100</sup>. The government and monetary multipliers, as representations of either theory, are thus seen in this context.

In considering some of the issues the above point can be clarified. Concerning the impact of alternative policies, the values of the fiscal and monetary multipliers in single-equation tests, on the one hand, are the result of the impact of a change in an independent variable (meaning a variable that is unrelated to other subsectors) on an endogenous variable. For the structural model, on the other hand, the variables in question are defined within a broader context and their linkages are traceable to a wider structure. In this sense simulation results of alternative policies reflect all these linkages and account for the totality of the system. Another example is the appropriate policy indicator issue. This issue is about the most correct indicator for the effects of monetary policy. For Fiscalists the effects of monetary policy are gauged in terms of interest rates, whereas for Monetarists the effects are calibrated in terms of the quantity of money<sup>101</sup>. A straightforward problem it seems, which could, presumably, be solved by an appeal to evidence constructed from tests of the effects of monetary policy (as measured either by money or interest rate) on economic activity. Both types of tests do this but the interpretation differs. In

addition, the results found in either test conflict with each other<sup>102</sup>. For the single-equation tests the variables are chosen irrespective of the specification of the complete structure, whereas large-scale models "concentrate on the specified causal links embodied in their structures"<sup>103</sup>. In essence, the two opposing factions espouse their own method of testing their theories with reality, and consider their own evidence as the most appropriate.

Notwithstanding all these methodological differences between the two tests, comparisons can be made between the two sets of evidence relating to the controversy, mainly for two reasons: (a) there are monetarist models, like the FRB-St.Louis model, the structure of which, although small in scale resembles the structure of economy-wide models, (b) despite fiscalist assertions A-J claim that tests can be considered reduced forms and that attempts have been made to specify the structure behind the tests<sup>104</sup>. In comparing the two tests we shall see however, that the empirical evidence is contradictory, the monetarist version of large-scale testing supports Monetarism, while the fiscalist complex testing supports Fiscalism.

b. FEATURES AND STRUCTURE OF THE FINANCIAL SECTOR

I shall briefly outline the structure of the financial sector of six econometric models as this will, firstly, provide a context within which to look at the empirical results and, secondly, enable the reader to contrast the two different approaches discussed in the previous section.

With the exception of the FRB-St.Louis model, all the models listed in Table I follow the fiscalist tradition and rely heavily on interest rates for the transmission of monetary impulses. Most of them use unborrowed reserves, the discount rate and open market operations as exogenous monetary policy instruments. In addition the MPS model uses the monetary base and the FRB-St.Louis model uses one definition of money. Although the latter uses a measure of fiscal policy (high-employment budget) it does not use a measure for tax changes. In Table I I have put in a summary form the most important characteristics of six econometric models.

The main objectives of these models are to make forecasts and to evaluate alternative policy actions. These objectives are shared among models in varying proportions. For example the Condensed Brookings model was considered an advance over the older Brookings models because it put the emphasis on policy simulations rather than forecasting. In reaction to monetarist criticisms relating to the 'cost of borrowing' limited channel of monetary influence, the FRB-MIT model expanded the financial sector and had as a specific objective to fully specify the transmission mechanism of monetary policy and provide a link with the real sector<sup>105</sup>. The major channels specified were the cost of capital, the rates of return on bonds, and credit rationing<sup>106</sup>. The Wharton Mark III model shares similar objectives, i.e. to expand the financial sector and to include a detailed treatment of prices<sup>107</sup>. The Liu-Hwa model offers an interesting feature that departs from the rest of the models. In this model they use a real rather than a nominal rate of interest.

TABLE I: SUMMARY OF SELECTED CHARACTERISTICS OF MODELS

MODEL	TIME FRAME	SCALE <sup>a</sup>	DISAGGREGATION OF PRODUCTION <sup>b</sup>	ENDOGENOUS FINANCIAL - REAL INTERACTION <sup>c</sup>	NO. OF ENDOGENOUS FINANCIAL VARIABLES
CONDENSED BROOKINGS	QUARTERLY	LARGE	MEDIUM	MEDIUM	7
FRB-MIT: D-8	QUARTERLY	MEDIUM			19
WHARTON MK III	QUARTERLY	VERY LARGE	MEDIUM	MEDIUM	11
LIU-HWA-71	MONTHLY	MEDIUM	LIMITED	MEDIUM	6
MPS (FED-MIT)	QUARTERLY	LARGE	LIMITED	STRONG	30
FRB-ST.LOUIS	QUARTERLY	VERY SMALL	NONE	STRONG	2

- NOTES:
- a. Based on number of equations: very small = 9 or less  
small = 10 - 49  
medium = 50 - 119  
large = 120 - 199  
very large = 200 or more.
  - b. Based on sector detail: limited = 2 - 5 sectors  
medium = 6 - 20 "  
high = 21 or more "
  - c. Based on qualitative judgements on pervasiveness of financial variables in real sector equations and real variables in financial sector equations (see Fromm and Klein, below).

SOURCES: G. Fromm and L.R. Klein, "The NBER/NSF Model Comparison Seminar: An Analysis of Results", Annals of Economic and Social Measurement, 1976, p.2, Fromm, "Survey of US Models . . .", op.cit., pp.388-391, Fisher and Sheppard, "Interrelationships . . .", op.cit., pp.211-5, and L.G. Andersen and L.C.A. Carlson, "A Monetarist Model of Economic Stabilization", FRB of St. Louis Review, Nov. 1970, pp.7-21.

This is in answer to the monetarist criticism concerning the Fiscalists' use of nominal interest rates which, assuming disequilibrium between real and nominal magnitudes, biases the results of econometric models<sup>108</sup>.

This point is important as it implies the need for a sound price-wage specification. The price-wage equation is, in fact, a weak point in model building. As Fromm and Taubman confirm,

An examination of the complete model solutions for 1961-62 [of the Brookings model] reveals that the wages and prices sector is one of the largest contributors of errors in the aggregate results.<sup>109</sup>

If a price-wage equation is properly specified then it is possible to incorporate price expectations. This in turn allows for the examination of situations whereby nominal interest rates diverge from real ones. The calibration of monetary policy with nominal interest rates is, therefore, biased when an inflationary period is studied. However, apart from the Liu-Hwa and the MPS models, the price-wage equation has been poorly specified by the rest of the models. For this reason the MPS model is important for the controversy, and also because it has as a specific objective the examination of the influence of monetary policy actions on the economy, within a complex model context. According to Ando the MPS model is sufficiently comprehensive to account for most questions of stabilization policies. Moreover it provides a theoretical framework that accommodates most of the Monetarists' contentions<sup>110</sup>. In addition it offers a highly disaggregated monetary sector with the greatest number of financial

exogenous variables. Finally the St. Louis model was built with the sole purpose to estimate the impact of alternative policies within a monetarist context.

In judging evidence from large-scale models, Monetarists offered a series of criticisms. One of these was the alleged confusion between nominal and real magnitudes. Another criticism related to the interaction between financial and real sectors. Implicit in this criticism was the assumption that Fiscalists' models lack a 'wealth effect', i.e. the impact of a change in prices on the relative yields of assets in private portfolios. The change in the yields induces a reshuffle of the assets with the consequence of changes in the pattern of private expenditures. For older models this was certainly true. However, for the new ones, like the MPS model "Consumption expenditures are dependent on stock market prices (through a wealth effect) and outlays for durables are tied to corporate bond rates"<sup>111</sup>. In addition housing starts equations and interest rates on fixed-plant and equipment are incorporated. Finally, there is the criticism of the 'budget-restraint'. It is claimed that models do not account for the effect of changes in the government debt payments for maturing securities. However, the Brookings and the FRB-MIT:D-8 models have partly accounted for this effect by introducing some of the relevant variables<sup>112</sup>.

Having roughly outlined the structure of the financial sector in a small sample of large-scale econometric models, the scene has been set wherein it is possible to examine the empirical evidence intended to resolve the Monetary controversy.

## c. THE EMPIRICAL EVIDENCE

Although the debate is not confined to a single issue but rather it consists of a range of controversial points (such as monetary versus fiscal policy, crowding-out effect, role of interest rate, stability of velocity, time-lag in effect of policy actions, exogeneity of money, indicators issue, etc.), it seems more relevant to concentrate on one issue, namely the effects of monetary and fiscal policy on macro-aggregates. This is due to two reasons: firstly, because this issue features as the most prominent one in the debate, generating much heated discussion, and secondly, because most of the empirical testing concentrates around this issue. The monetarist position is that money plays a more important role than government expenditures, while the fiscalist position is that both monetary and fiscal policies are important. However, the hypothesis to be tested is not about the absolute superiority of either fiscal or monetary policy, but rather about the speed of response and the dynamic implications of each policy. Monetarists claim that changes in the money stock affect real output in the short-run and nominal magnitudes in the long-run. Real output in the long run is affected by real forces, such as productivity trends. On the other hand, Fiscalists claim that changes in government expenditures and taxes have an important influence - along with accommodating monetary policy - on both real and nominal magnitudes in the short and long run.

In Tables II, III, V, VI, VII, VIII, and IX I have set out the results from empirical testing of a sample of models, of the power of fiscal and monetary policy in influencing economic activity. Although,

strictly and technically speaking, it is illegitimate to make comparisons of results, due to different initial conditions (most of the models are non-linear) and somewhat different specifications, yet it is still legitimate to make the comparisons on the assumption that differences in initial conditions and specifications have a small effect on the direction and magnitude of the multipliers, and they are not so great as to make comparisons impossible. In his survey of large-scale models, Fromm reports that, "some of the disparities are great. Yet interchanges between model builders have resulted in strikingly similar treatment in a number of areas"<sup>113</sup>.

Most of the large-scale models are in agreement about the effects of fiscal policy (see Table II). The values of the fiscal multipliers range from 2 to 3 in the first four quarters and increase thereafter. In the tenth year the multipliers range from 3.3 to almost 4.0<sup>114</sup>. However, when the increase in the price level is taken into account the results somewhat differ. In the beginning the differences are not so great, values range from 2 to 2.7, however, afterwards they start to decrease reaching in the tenth year, for the Brookings model 0.9 and for the Wharton -3.0. For the FRB-St.Louis, however, the value of -0.2 had been reached from the eighth quarter and remained so<sup>115</sup>. The values of the personal tax multipliers are somewhat lower than the expenditure ones. This, however, according to Fiscalists, is not unexpected.

The difference between expenditure and tax multipliers need not necessarily equal unity. They do so only in simplistic balanced-budget models that exclude a multiplicity of leakages and income-expenditure feedbacks.<sup>116</sup>

TABLE II: FISCAL POLICY DYNAMIC MULTIPLIERS

GOVERNMENT NONDEFENSE EXPENDITURES ON GNP (current dollars)							
QUARTER	Condensed Brookings	Wharton Mk III	MPS (Fed-MIT)	FRB-St. Louis		Liu-Hwa monthly	
	CUR <sup>a</sup> CON <sup>b</sup>	CUR CON	CUR CON	CUR	CON	CUR	
1	1.8 1.8	1.2 1.3	1.2 1.2	0.6	0.5	1.1	
2	2.3 2.4	1.5 1.6	1.7 1.5	1.1	1.0	1.5	
3	2.7 2.7	1.7 1.8	2.1 1.9	1.2	1.0	1.8	
4	2.8 2.8	1.8 2.0	2.5 2.2	0.7	0.5	2.1	
8	2.9 2.7	2.2 2.4	3.0 2.2	0.1	-0.2	-	
PERSONAL TAX ON GNP							
1	1.0 1.0	0.4 0.5	0.4 0.4			0.1	
2	1.4 1.3	0.7 0.8	0.9 0.8			0.4	
3	1.6 1.5	0.9 1.0	1.2 1.1			0.9	
4	1.8 1.6	1.0 1.2	1.5 1.3			1.2	
8	2.3 1.6	1.5 1.7	2.7 2.1			-	

## NOTES:

a. CUR = current dollars

b. CON = constant dollars (1958)

Brookings: Increase of \$5 billion (1958) dollars) in  
 Period 1956:1 - 1965:4. government expenditure; decrease of  
 \$5 billion in personal taxes.

Wharton Mk III: Increase of \$1 billion in nondefense  
 Period 1965:1 - 1974:4. expenditures with average associated change  
 in government wage bill and employment;  
 decrease of \$1 billion in personal taxes.

MPS: \$1 billion increase in exports without  
 accommodating monetary policy and \$1 billion  
 decrease in personal taxes.

FRB St.Louis: \$5 billion increase in nondefense  
 Period 1962:1 - 1966:4. expenditures.

Liu-Hwa: Increase of \$5 billion in nondefense  
 Period 1961:01 - 1964:06. spending.

SOURCE: Fromm, "Survey of United States Models", op.cit., p.408, and  
 G. Fromm and L.R. Klein, "A Comparison of Eleven Econometric  
 Models of the United States", American Econ.Review, 6, 1973,  
 pp.391-392.

In general most of the models agree that fiscal policy is quite powerful and economic aggregates respond quite sensitively to its changes<sup>117</sup>. This can be clearly seen in Table III whereby comparisons are made of results concerning the effects of fiscal actions between a large-scale model and a small-scale model.

TABLE III: THE EFFECTS ON GNP OF FISCAL POLICY IN  
THE FRB-MIT AND ST. LOUIS MODELS<sup>a</sup>

EFFECTS AFTER	FRB-MIT MODEL <sup>b</sup>		ST. LOUIS MODEL	
	SPENDING	TAXING	SPENDING	TAXING
Quarter				
1	2.0	1.1	0.36	-0.16
2	2.5	2.2	0.89	-0.15
3	3.4	3.2	0.06	-0.23
4	3.2	4.7	0.06	-0.23

- NOTES: a. Based on \$1 billion spending increase or tax decrease, or equivalent. Initial conditions of 1963:1 are used in the FRB-MIT model. The FRB-MIT results are converted into comparable dimensions with those of the St. Louis model.
- b. Full model effects.

SOURCE: Teigen, "The Keynesian-Monetarist Debate in the U.S. . . .", op.cit., p.24.

Whereas in the fiscalist model empirical evidence shows an appreciable effect of expansionary fiscal policy on GNP, in the monetarist model fiscal policy is shown to be impotent. Within the St. Louis model fiscal policy has almost adverse impacts on economic activity. More

recent simulations with the MPS model, however, corroborate the fiscalist results and oppose the monetarist evidence. Having described evidence of a demand shock in the model Modigliani concludes that,

These results, which are broadly confirmed by other econometric models . . . [do not] support the monetarist view of a highly stable economy in which shocks hardly make a ripple and the effects of fiscal policy are puny and fast vanishing.<sup>118</sup>

On the other hand, as can be seen from Table III, the St. Louis model shows different evidence. It shows that fiscal actions have a lesser impact, and from the fourth quarter their influence starts to diminish. Andersen and Carlson report that,

According to the model fiscal actions have short-run effects, but for period of a year or more . . . the net effect is much smaller . . . monetary actions are the major factor contributing to economic fluctuations.<sup>119</sup>

It should be noted that the FRB-St.Louis model was extensively tested and it was found that its performance, as measured by its forecasting record was, at the least, comparable to the performance of large-scale models<sup>120</sup>. In Table IV we can compare the predictive performance of the St. Louis model with the Wharton and see that the former does as well as the latter.

Regarding the effects of an expansionary monetary policy on the economy, on the one hand results from large-scale models conform to Fiscalists' expectations that both monetary and fiscal policies are important, whereas results from small-scale models conform to

TABLE IV: FORECASTING PERFORMANCE OF TWO MODELS

NAME OF MODEL	ROOT MEAN SQUARE ERRORS			
	NOMINAL GNP	REAL GNP	PRICE LEVEL	UNEMPLOYMENT
WHARTON	2.00	2.92	0.33	1.28
ST. LOUIS	1.49	3.09	0.60	0.21

Sample periods: Wharton: 1948 - 1964  
 St. Louis: 1956 - 1969

Exogenous variables: Wharton: 43  
 St. Louis: 3

SOURCE: Andersen and Carlson, "A Monetarist Model for Economic Stabilization, op.cit., p.16.

Monetarists' expectations that monetary policy - i.e. a fixed change in money and not fine-tuning - has a relatively stronger effect on the economy than fiscal measures. In Table V we can see the effect of a \$1 billion increase of unborrowed reserves on the levels of GNP, prices, and on the short- and long-term interest rates<sup>121</sup>.

For the CB model an increase of money has an adverse effect on GNP for the first quarter, while it increases slowly (and perhaps erratically) thereafter. For the FRB-MIT model, however, GNP increases steadily from the first quarter. An expansion of the money supply has the expected (for Fiscalists) sign for both short- and long-term interest rates in the CB model. These results suggest that for the CB model, and to a certain extent for the FRB-MIT model,

TABLE V: MONETARY POLICY SIMULATIONS WITH TWO LARGE-SCALE MODELS:  
INCREASE OF \$1 BILLION IN UNBORROWED RESERVES

MODEL	QUARTER	REAL GNP <sup>a</sup>	GNP DEFLATOR	TREASURY BILL RATE %	BOND RATE
CONDENSED BROOKINGS (initial condition 1960:2)	1 5	-1.5 5.1	0.2 -0.6	-1.48 -0.39	-0.70 -0.26
	2 6	2.7 11.6	-0.4 -1.2	-0.58 -0.50	-0.11 -0.33
	3 7	2.6 8.9	-0.3 -0.7	-0.42 -0.39	-0.19 -0.29
	4 8	4.5 8.9	-0.5 -0.7	-0.42 -0.40	-0.23 -0.31
FRB-MIT: D-8 (initial condition 1963:1)	1 5	0.7 7.0	0.0 0.1		
	2 6	2.0 8.3	0.0 0.3		
	3 7	3.6 9.3	0.1 0.4		
	4 8	5.4 10.0	0.2 0.6		

NOTE: a. 1954 \$bn for the CB model and 1958 \$bn for the FRB-MIT model.

SOURCE: Fisher and Sheppard, "Interrelationships . . .", op.cit., p.226.

an increase in the money supply, although important, does not have as strong an effect on the economy as the one suggested by Monetarists.

However, results from monetarist models show that an increase in the growth rate of the money supply has the expected effect hypothesized by Monetarists<sup>122</sup>. Table VI shows results from a projection of a 6 per cent growth in the money stock on target variables.

TABLE VI: SIMULATIONS OF A PROJECTED 6 PER CENT INCREASE IN THE MONEY STOCK WITH THE FRB-ST.LOUIS MODEL

QUARTER	IV/1969	I/1970	II/1970	III/1970	IV/1970	I/1971	II/1971	III/1971
REAL GNP	-0.1	-1.3	-0.1	0.2	1.6	3.3	3.7	4.8
NOMINAL GNP	5.1	3.5	4.6	4.8	6.0	7.6	7.8	8.9
GNP PRICE DEFULATOR	5.0	4.9	4.7	4.5	4.3	4.2	4.0	3.9
COMMERCIAL PAPER RATE	8.0	7.6	6.7	6.5	6.3	6.1	5.9	5.7
CORPORATE RATE	7.4	7.4	7.2	7.2	7.3	7.3	7.3	7.2

SOURCE: Andersen and Carlson, "A Monetarist Model for Economic Stabilization", op.cit., p.19.

The projected simulations show that total spending would be increasing at almost 9 per cent in the eighth quarter. Although output will decline very slowly at the beginning, reflecting the effects of past restrictive monetary and fiscal actions<sup>123</sup>, it will start increasing from the second quarter to reach an almost 5 per cent growth rate by the end of the second year. Past restrictive monetary actions will also retard the increase of the price level. However, even with this, the inflation rate will be almost 4 per cent by the end of 1971<sup>124</sup>. Finally, although both short- and long-term interest rates will start falling this would be in response to the temporary reduction in output, and it would not correspond to the rapid growth of money<sup>125</sup>. However,

the fall of the interest rates will be slow compared to that indicated in large-scale models. From what it seems then from these results, despite restrictive past economic policies and inflation, the model seems to corroborate the monetarist hypothesis that an increase in the money stock will have a pronounced effect on the economy. This can also be seen in Table VII where comparisons are made between results from three fiscalist and one monetarist model.

TABLE VII: DYNAMIC MULTIPLIERS: GROSS NATIONAL PRODUCT/UNBORROWED RESERVES OR MONEY STOCK (GNP/'M CURRENT DOLLARS)

QUARTERS	FRB-ST. LOUIS	MPS	WHARTON MK III	L1U-HWA (monthly)
1	1.2	0.4	1.9	0.0
2	2.9	1.1	2.4	0.1
3	4.5	2.2	3.4	0.6
4	5.3	3.6	4.1	1.5

FRB-ST.LOU1S:  
Period 1962:1 - 1964:4

Increase of 0.5 billion  
in MI

MPS:

Increase of 0.5 billion  
in unborrowed reserves

WHARTON MK III:  
Period 1965:1 - 1974:4

Increase of 0.5 billion  
in unborrowed reserves

LIU-HWA:  
Period 1961:01 - 1964:06

Increase of 1 billion  
in unborrowed reserves

SOURCE: Fromm and Klein, "The NBER/NSF Model Comparison Seminar", op.cit., pp.25-26.

Thus empirical evidence as determined by the two alternative approaches seems to favour both sides. This, however, cannot be seen straightforwardly when comparing the two tables, since different experiments are involved. Nonetheless adjustments have been made and results have been converted from the FRB-MIT model in order to compare with the St. Louis model.

TABLE VIII: CHANGES IN NOMINAL GNP GENERATED  
BY A \$1 BILLION INCREASE IN  
THE MONEY STOCK<sup>a</sup>

EFFECT AFTER	FRB-MIT MODEL <sup>b</sup>	ST. LOUIS MODEL
1 quarter	\$ 0.2	\$ 1.5
2 quarters	0.4	3.1
4 quarters	0.4	5.8
12 quarters	2.2	5.8

NOTES: a. In billions of current dollars:  
initial conditions of 1963:1 for  
the FRB-MIT model

b. Based on operations of the full  
model.

SOURCE: Teigen, "The Keynesian-Monetarist Debate . . .",  
op.cit., p.19.

Table VII compares the effects of an increase in \$1 billion on total spending within the two models. Clearly, from what can be seen from the table total spending in the St. Louis model responds much more strongly and quickly than the FRB-MIT model.

Further comparisons between monetarist and fiscalist models have been made and they have shown the monetarist side to be much more sensitive to changes in money than the fiscalist side. Table IX compares two fiscalist models with the FRS-Chicago model which, though larger than the FRB-St.Louis model, is still within the monetarist framework. Whereas for the fiscalist models total spending increases slowly, for the FRS-Chicago it increases quickly reaching the value of almost 11 by the end of the year.

TABLE IX: CHANGE IN NOMINAL INCOME DUE TO \$1 BILLION INCREASE IN MONETARY POLICY INSTRUMENT<sup>a</sup>

QUARTER	FRS-MIT	FRS-CHICAGO-MIT	BROOKINGS
1	0.7	1.2	-0.8
2	2.1	3.7	1.3
3	4.3	7.2	1.6
4	6.7	10.8	2.9

NOTE: a. The policy instrument is unborrowed reserves (Brookings and FRS-MIT) total reserves (FRS-Chicago-MIT)

SOURCE: R. Zecher, "Implications of Four Econometric Models for the Indicators Issue", American Economic Review Papers and Proceedings, 1970, p.48.

In conclusion, one could say that for complex fiscalist models, although the results are more disparate than for fiscal policy,

monetary policy seems to be an important instrument, although not as important as fiscal policy (this in fact corresponds to the 'eclectic' position)<sup>126</sup>. While both policies are important, fiscal policy features more prominently within large-scale models. In contrast, results on the speed of response and the time paths of the alternative policies from monetarist models seem to support the opposite conclusion, namely that Monetary policy acts faster and more effectively than fiscal policy. Thus, as with single-equation tests, evidence from large-scale models is again inconclusive.

Econometric models have been criticized by many people on many grounds. They were criticized for leaning heavily towards Keynesian theory<sup>127</sup>, or for including a weak monetary sector and not separating nominal from real magnitudes while relying solely on the cost of borrowing channel of influence and not using appropriate monetary measures<sup>128</sup> or, finally, of large errors due to high disaggregation<sup>129</sup>. In fact nobody, not even the Fiscalists, accepted evidence from large-scale models as absolutely objective. "It would be ironic", says Samuelson, "if, inside the Wharton model, we found in the end, Lawrence Klein"<sup>130</sup>. Fiscalists themselves admitted to the fact that theoretical preconceptions enter into the specification of the equations<sup>131</sup>, and that in many circumstances forecasting with these models is not very different, although more reliable and indispensable, from ad hoc judgements<sup>132</sup>.

Notwithstanding inside and outside criticisms, the performance of the models was considered quite satisfactory and the results were accepted as reliable<sup>133</sup>. Although there were many reservations as to

the accuracy of the results, Fiscalists still believed in the results from large-scale models as providing objective evidence:

there is a[n] . . . advantage to a formalized model,  
i.e. the protection of objectivity and a safeguard  
against self-deception . . . Econometric models  
have unblemished records for being dispassionate.<sup>134</sup>

Despite the various defects in large-scale models, Fiscalists still believe in their ability to evaluate and test economic hypotheses and policies.

Thus, as far as Fiscalists are concerned, fiscalist theory and policy, according to the evidence, is shown to be correct. However, Monetarists on their part also have shown their theory and policy to be correct. Fiscalists have referred to empirical evidence from large-scale models confirming the validity of their thesis. Monetarists, on the other hand, appealed to empirical evidence that proved their thesis and conflicted with the Fiscalists' results. They used small-scale models with specifications that were geared towards Monetarism. The findings supported their hypotheses. Perhaps some convergence of views has been achieved regarding the "rediscovery" of money<sup>135</sup>. However, as has been noted, this was not the issue as far as Fiscalists are concerned. Early Keynesians might have relegated money to a minor position as a policy variable. But the 'sophisticated' Fiscalists accept the importance of both fiscal and monetary policy as instruments of macroeconomic management. As Modigliani says, "the reevaluation of the role of money reflects not rediscovery, but rather the notable strides we have made in peering into the monetarists' 'black box'"<sup>136</sup>. The bone of contention thus

seems to be not whether money is important or not but whether one should apply "a fixed . . . rule of monetary policy"<sup>137</sup> or a coordination of policies for 'fine-tuning' the economy. And regarding this issue both sides have found favour in empirical facts<sup>138</sup>. Both groups, in fact, appealed to empirical evidence, and both attempted to show the world 'as it is' without value judgements or preconceptions. Both, however, found 'hard facts' to support their theory and reject the other's. According to Ando, "the MPS Model . . . indicates that it will take roughly 30 to 40 quarters . . . before complete 'crowding-out' effects take place . . . Therefore the monetarist contention . . . has no foundation"<sup>139</sup>. And according to Andersen, "Fiscal actions . . . have only a transitory impact on economic activity . . . [whereas the St. Louis Model] [d]emonstrate[s] the importance of monetary actions in determination of movements in economic activity"<sup>140</sup>. The claims of both groups were scientifically, in the positivist sense, justified. Indeed, in a case like this, as Klein says, "what are we to believe?".

#### E. SUMMARY AND CONCLUSION

The purpose of this part of the thesis was to see whether, in a given case such as the Monetary controversy, empirical evidence can act as an arbiter between conflicting views. In order to do this I first examined the development of the controversy and the qualitative judgements bearing on the two alternative policy recommendations. Further, I briefly reviewed the theoretical framework of the controversy and indicated that Fiscalists and Monetarists do not differ in respect to the theoretical framework essentially but in respect to

different emphases and interpretations of the same framework.

Finally, having set the scene, I surveyed the empirical evidence that was meant to resolve the controversy. Firstly, I examined the monetarist empirical results stemming from single-equation tests and the fiscalist counter-evidence, and found that the controversy still remained unresolved. Secondly, I examined the fiscalist empirical results stemming from large-scale tests and the monetarist counter-evidence and found that not even here could the controversy find resolution. Thus, despite the production of empirical evidence from fiscalist and monetarist tests, the debate persists<sup>141</sup>.

It may be true, however, that the debate does not have the same form as before. Now Fiscalists are more reluctant to subscribe to a Radcliffe Committee view. Perhaps one might see this as a partial success of the power of empirical testing. But it is also true that Fiscalists never took the extreme position of the Radcliffe Report quite seriously. For instance, Samuelson claims that,

Most of the evidence is conclusive with respect to a Radcliffe Committee stupid view that money doesn't matter, but as to the view that money matters and that fiscal policy . . . does not . . . there is very little . . . evidence that is germane to that.<sup>142</sup>

As a matter of fact Fiscalists never doubted the importance of money as such. What they doubted was the absolute causality ascribed to money and the implied impotence of fiscal policy. "The issue", says Heller, "is not whether money matters - we all grant that - but whether only money matters, as some Friedmanites . . . would put it"<sup>143</sup>.

In this respect, therefore, Fiscalists remained firm in their view that both monetary and fiscal policy are important, while Monetarists rested confident on their view of the appropriate role of money.

So what has been the 'dispassionate' verdict of empirical evidence with respect to these two alternative views? Brainard and Cooper, for instance, say that,

Despite the emergence . . . of wide agreement about the importance of monetary policy, the empirical basis for many of our beliefs, and a fortiori for distinguishing among our differences, has remained weak.<sup>144</sup>

Also both Monetarists and Fiscalists feel quite disappointed about the reconciliatory power of empirical testing. For example Andersen admits that,

the debate is far from being resolved . . . Empirical evidence presented to date has proven to be inconclusive - there is support for both sides of the debate.<sup>145</sup>

And Modigliani adds that,

despite broad agreement of principle [differences] would still lead, say Friedman and myself to advocate much of the time, including right now, very different monetary and fiscal policies.<sup>146</sup>

Thus the controversy still persists among Positive economists presently<sup>147</sup>. Empirical testing, despite its 'positive', 'dispassionate' status, has done very little to lift the burden of the conflict from the shoulders of orthodox economics<sup>148</sup>. This irresolution is manifested in the extent to which economists deal with the issues persistently. For instance, in 1970 there was a conference on the

indicators issue published in the American Economic Review<sup>149</sup>. Also in 1971 Fiscalists expressed their views on "eclecticism" in a collection of lectures presented at De Paul University<sup>150</sup>. In 1975 there was an open debate between Friedman and several British economists on policy issues shown on television. Even after Friedman's Nobel prize presentation there followed a debate again shown on television. Concerning the economic prospects for 1977 in the United States, economists are still divided between a fiscalist and a monetarist view<sup>151</sup>. Also in relation to the causes of inflation the gap is even more widened<sup>152</sup>. Finally in journals such as the Federal Bank of St. Louis Review, the Lloyd's Bank Review, the International Economic Review and the American Economic Review the debate is going on, perhaps not as strongly as before, but at a firm pace<sup>153</sup>.

However, how does this inability of empirical testing to resolve the controversy and convince the opposing factions reconcile with the Positive economic methodological dictum that empirical evidence should choose between competing theories? How can Positive economists be faced with the fact that the dictum does not work, at least as far as the Monetary controversy is concerned, and still believe in Positive economics? Obviously, Monetarists cannot argue that Fiscalists do not accept the empirical facts because of blind Keynesian ideology, because Fiscalists have produced their own empirical facts, which of course are not accepted by Monetarists. Why then empirical results cannot convince the opposing sides when both, in fact, accept the rule of empirical testing as the only criterion of truth? As Foley says, "The ineffectiveness of policy economics is a symptom of a deeper bind that economists are in"<sup>154</sup>.

## FOOTNOTES

## PART II: CHAPTER 5:

1. "Empirical Evidence on Fiscal and Monetary Models", op.cit., p.36.
2. As Coddington says, "the recent controversies in monetary theory . . . have not been brought any nearer to resolution by the introduction of evidence by either side, since part of the controversy in each case concerns the status and interpretation of such evidence; that is to say, what, if anything, is to count as evidence for", "Positive Economics", op.cit., p.9.
3. Walters, "The Radcliffe Report", op.cit., p.40, and esp. pp.39-46.
4. Quoted in ibid., pp.40-41.
5. See, for example, D. Laidler, "The Rate of Interest and the Demand for Money - Some Empirical Evidence", Journal of Political Economy, 1966, pp.55-68. Related to this issue is also the question of the stability of monetary velocity as reflecting the importance of the quantity theory, and the instability of velocity as reflecting the importance of the fiscalist theory.
6. However, as Meiselman observes, "the difference between the two research strategies is not accurately captured by the difference between single equations and simultaneous equation estimates but rather by the level of abstraction and the complexity of the interrelations imposed on the structure of the model", Varieties of Monetary Experience, 1970, p.6.
7. For a full discussion of this point see Carlson, "Monetary and Fiscal Actions in Macroeconomic Models", op.cit., esp. pp.12-14.
8. Friedman, "Money and Economic Development", op.cit., pp.9-10.
9. The reliability and objectivity of Monetarist evidence was disputed by Brunner and Meltzer who claimed that, "The empirical work done by Friedman, Meiselman . . . Keran . . . Andersen and Jordan . . . and others . . . provides no evidence that changes in the government expenditure and taxation have no effect on output", "Friedman's Monetary Theory", op.cit., p.842.
10. M. Friedman and A.J. Schwartz, A Monetary History of the U.S., 1867-1960, 1963.
11. See Johnson, "Recent Developments in Monetary Theory", op.cit., pp.87-8. The money-income correlation evidence was also substantiated by the estimation of a model rationalizing the correlation in an article written by Friedman, "Money and Business Cycles", Review of Economics and Statistics, Feb.1963, pp.32-64.
12. Kaldor, "The New Monetarism", op.cit., p.10.

## FOOTNOTES (cont.)

## PART II: CHAPTER 5:

13. Kaldor, "The New Monetarism", op.cit., p.11.
14. Samuelson, "The Role of Money", op.cit., p.11.
15. R. Davis, "Discussion", American Economic Review, 1969, p.316.
16. J. Tobin and C. Swan, "Money and Permanent Income: Some Empirical Tests", American Economic Review, 1969, Papers and Proceedings, p.294. It is important to note in this context that Tobin and Swan's estimation of Friedman and Schwartz's model seem to follow this model quite closely; for instance Davis in assessing Tobin and Swan's estimation concludes that, "Tobin and Swan's procedures seem to follow the Friedman-Schwartz program faithfully, and their very negative verdict on the result seems wholly justified.", ibid., p.315. (However, they applied a variant of Friedman and Schwartz's equation on quarterly data. Friedman and Schwartz used longer-run data. For the justification and a reconciliation see Tobin and Swan, ibid., pp.387-8; for a discussion of the modifications used by Tobin and Swan see Davis, ibid., p.315.)
17. Tobin and Swan, ibid., pp.391-3.
18. Ibid., p.289.
19. Ibid., p.288.
20. A.H. Meltzer, "The Role of Money in National Economic Policy", in Controlling Monetary Aggregates, I, 1969, op.cit., p.26.
21. D. Meiselman, "The Role of Money in National Economic Policy", ibid., p.15.
22. M.J. Artis and A.R. Nobay, "Two Aspects of the Monetary Debate", National Institute of Economic Review, 1969, p.35.
23. J. Tobin, "The Role of Money in National Economic Policy", in Controlling Economic Aggregates, 1969, op.cit., p.21. Also Davis claims that, "the Friedman-Schwartz equation does work quite badly as applied to postwar data", ibid., p.315.
24. Friedman, "Money and Economic Development", op.cit., p.17.
25. Samuelson, "The Role of Money . . .", op.cit., p.9. Hicks also adds that a "statistical correlation does not prove causality. It does no more than make it probable that there is a relation between the variables, but the relation does not have to be a direct relation of cause and effect", J. Hicks, "What is Wrong with Monetarism", Lloyd's Bank Review, 1975, p.3. See also Laidler, "The Influence of Money on Economic Activity", op.cit., p.87.

## FOOTNOTES (cont.)

## PART II: CHAPTER 5:

26. For an example of how interest rate movements influence the movements in the money supply Kern says that, "the wide differentials between market rates and base rates in 1972 and 1973 caused very sharp 'statistical' increases in the money supply which did not bear any relationship to the underlying economic factors", D. Kern, "Monetary Aspects of the Current Economic Debate", National Westminster Bank Quarter Review, 1975, p.14. Harrington also adds that, "In an economy at times when the rate of interest was free to vary the quantity theory of money would have been a good approximation to reality. In any economy in which the rates of interest were not completely free to vary then much of what the anti-monetarists have alleged would be true", R.L. Harrington, "The Monetarist Controversy", Manchester School, 1970, p.299.
27. Davis, ibid., p.316.
28. Friedman, "A Theoretical Framework . . .", op.cit., p.210.
29. M. Friedman and D. Meiselman, "The Relative Stability of Monetary Velocity and the Investment Multiplier in the U.S. 1897-1958", in E.C. Brown, et al., Stabilization Policies. A Series of Research Studies Prepared for the Commission on Money and Credit, (Englewood Cliffs, N.J.), 1963, 1964, pp.165-268.
30. Ibid., pp.187-8.
31. Ibid., p.188. "The income velocity of circulation of money is consistently and decidedly stabler than the investment multiplier except only during the early years of the Great Depression after 1929", ibid., p.186. The average correlation coefficients for the whole period were 0.98 for the money equation and 0.76 for the autonomous expenditures equation.
32. A. Ando and F. Modigliani, "The Relative Stability of Monetary Velocity and the Investment Multiplier", American Economic Review, 1965, pp.692-728, M. De Prano and T. Meyer, "Tests of Relative Importance of Autonomous Expenditures and Money", American Economic Review, 1965, pp.729-52, and D.D. Hester, "Keynes and the Quantity Theory: A Comment on the Friedman-Meiselman CMC Paper", Review of Economics and Statistics, 1964, pp.364-8.
33. Hester, ibid., p.364.
34. Ibid., p.707.
35. Ibid., p.737.
36. Ando and Modigliani, ibid., p.696 and p.698, and also Hester, ibid., p.368.

## FOOTNOTES (cont.)

## PART II: CHAPTER 5:

37. D.D. Hester, "Keynes and the Quantity Theory: A Comment on the Friedman-Meiselman CMC Paper", Review of Economics and Statistics, 1964, p.368.
38. A. Ando and F. Modigliani, "The Relative . . .", op.cit., pp.693-4.
39. Ibid., p.694. While talking of the crudity of the tests, Ando and Modigliani claim that, "It is precisely the endeavour to take into account these more complex interrelations that has led to the formulation of more or less extensive models", ibid., p.703.
40. W. Poole and E. Kornblith, "The Friedman-Meiselman CMC Paper: New Evidence on an Old Controversy", American Economic Review, 1973, p.915.
41. Other criticisms also included arguments such as that due to fluctuations in the state of the economy, the results of the F-M tests could not be taken as policy implications, since even if velocity had been proved relatively stable this did not mean that it would remain stable in the future (see Bank of England, op.cit., p.30). Or arguments such as that the data on which the fiscalist hypothesis was tested was full employment data and since the simple Keynesian model is not a theory of "the determination of the level of money income at full employment . . . The simple 'Keynesian' model should not be applied to such [full employment] data", Laidler, "The Influence of Money on Economic Activity", op.cit., pp.85-87. Insofar as the quality of data is concerned Ando and Modigliani claimed that "obtaining high correlations is not much of a trick when dealing with U.S. time-series data", ibid., p.714, footnote 25.
42. Ando and Modigliani, ibid., p.707.
43. Ibid., pp.693-4 and p.727.
44. Hester, ibid., p.364.
45. Ibid., p.367.
46. Ibid., p.368.
47. Ibid., p.368.
48. De Prano and Mayer, op.cit., p.747.
49. Ando and Modigliani, op.cit., p.716 and p.694.
50. Ibid., p.708, also De Prano and Mayer, ibid., p.745, and Hester, ibid., p.364.

## FOOTNOTES (cont.)

## PART II: CHAPTER 5:

51. Friedman and Meiselman, "Reply to Ando and Modigliani and to De Prano and Mayer", American Economic Review, 1965, p.785.
52. Ibid., pp.762-4.
53. Ibid., p.754. Or they argued that, "Hester gives no empirical evidence to support his own stated preferences among alternative definitions", in, "Reply to Donald Hester", Review of Economics and Statistics, 1965, p.370, and that, "A-M [Ando and Modigliani] and D-M [De Prano and Mayer] give no evidence, theoretical or empirical, that we have used the wrong criterion in selecting A [autonomous expenditure] or that we interpreted the empirical evidence we present incorrectly", "Reply to Ando . . .", op.cit., p.763.
54. Ibid., p.760.
55. Ibid., p.785.
56. Hester, "Rejoinder", Review of Economics and Statistics, 1965, p.377.
57. Ando and Modigliani, "Rejoinder", American Economic Review, 1965, pp.786-7.
58. C.R. Barret and A.A. Walters, "The Stability of Keynesian and Monetary Multipliers in the United Kingdom", Review of Economics and Statistics, 1966, pp.395-405.
59. For Barret and Walters up to almost the First World War and perhaps up to 1920, money seemed to have had the predominant role. The inter-war years were significantly Keynesian and the explanation of the post-war years was distributed to both models.
60. V. Argy, "The Role of Money in Economic Activity: Some Results for Seventeen Developed Countries", IMF Staff Papers, 1970, p.527.
61. Friedman and Schwartz, "Monetary Statistics of the U.S.", op.cit., p.2<sup>2</sup>. See also p.1.  
10
62. Johnson, "Macroeconomics and Monetary Theory", op.cit., p.123. See also Walters, "The Radcliffe Report . . .", op.cit., p.49.
63. D.G. Pierce and D.M. Shaw, "A Review of the Empirical Evidence Relating to the Role of Money and the Effectiveness of Monetary Policy", Monetary Economics: Theories, Evidence and Policy, 1974, p.237.
64. Ibid., p.237.
65. Fisher and Sheppard, "Interrelationships . . .", op.cit., p.218.

## FOOTNOTES (cont.)

## PART II: CHAPTER 5:

66. W.C. Brainard and R.N. Cooper, "Empirical Monetary Macroeconomics: What Have We Learned in the Last 25 years", American Economic Review, 1975, p.169.
67. Friedman, "Money and Economic Development", op.cit., pp.1-9.
68. Carlson, "Monetary and Fiscal Actions in Macroeconomic Models", op.cit., pp.12-14.
69. L.C. Andersen and J.L. Jordan, "Monetary and Fiscal Actions: A Test of their Relative Importance in Economic Stabilization", FRB of St. Louis Review, 1968, pp.11-24.
70. See footnote 93.
71. See Teigen, "The Keynesian Monetarist Debate in the U.S. . . .", op.cit., p.7.
72. See P.S. Anderson, "Monetary Velocity in Empirical Analysis", in, Controlling Monetary Aggregates, I, 1969, op.cit., p.39.
73. Andersen and Jordan, in "Monetary and Fiscal Actions: A Test of Their Relative Importance in Economic Stabilization-Reply", FRB St. Louis Review, April 1969, p.16.
74. Friedman, "A Theoretical Framework . . .", op.cit., p.47.
75. See, e.g. Teigen, "The Keynesian-Monetarist Debate . . .", op.cit., p.9, and E.M. Gramlich, "The Role of Money in Economic Activity: Complicated or Simple", Journal of Business Economics, 1969, pp.21-26.
76. F. De Leeuw and J. Kalchbrenner, "Monetary and Fiscal Actions: A Test of Their Relative Importance in Economic Stabilization-Comment", FRB of St. Louis Review, April, 1969, pp.6-8.
77. Teigen, ibid., p.10.
78. Artis and Nobay, "Two Aspects of the Monetary Debate", op.cit., pp.37-42.
79. Walters, "The Radcliffe Report . . .", op.cit., pp.51-54.
80. R.G. Davis, "How Much Does Money Matter? A Look at Some Recent Evidence", FRB of New York Monthly Review, June 1969, pp.119-131.
81. De Leeuw and Kalchbrenner, ibid., pp.6-11.
82. Ibid., p.7.
83. Ibid., p.8.

## FOOTNOTES (cont.)

## PART II: CHAPTER 5:

84. The monetary base is defined as demand deposits plus currency held by the public outside the banks.
85. D-K, "Comment", op.cit., p.10. For the results see p.9.
86. Ibid., p.11.
87. Ibid., p.11.
88. A-J, "Reply", op.cit., p.12.
89. Ibid., p.13.
90. Ibid., p.16.
91. ". . . if the statistical constraints are removed, estimation of the St. Louis equation indicates that money matters, but so do fiscal actions . . . if the variable employed to measure monetary actions is adjusted to account for 'reverse causation', the conclusion derived from the St. Louis equation is that money still matters, but again, so do fiscal actions. In spite of these criticisms, the St. Louis equation continues to be used in its original form", R.S. Koot, "On the St. Louis equation and an Alternative Definition of the Money Supply", The Journal of Finance, 1977, p.917.
92. "Empirical Monetary Macroeconomics", op.cit., p.170.
93. However, Monetarists have attempted to specify a structure, though not as complex as that of the Fiscalists', from which the A-J tests could be considered reduced forms. See A. Walters, "Discussion Paper", in Johnson and Nobay, "Issues in Monetary Economics", op.cit., p.8, also L.C. Andersen, "The State of the Monetarist Debate", FRB of St. Louis Review, Sep.1973, p.8, and L.C. Andersen, "A Monetary Model of Nominal Income Determination", FRB of St. Louis Review, June 1975, pp.9-17.
94. G. Fromm and P. Taubman, "Policy Simulations with an Econometric Model", 1968, p.IX. See also S. Golberger, Impact Multipliers and Dynamic Properties of the Klein-Golberger Model, 1959, pp.1-3, and L.R. Klein and A.S. Goldberger, An Econometric Model of the United States 1929-1952, 1955, p.VII. Klein also adds that "the true understanding of the economic process is much more complicated than suggested by simple models", in Diamond, "Issues . . .", op.cit., p.50.
95. Friedman, "The Methodology of Positive Economics", op.cit., pp.30-32.
96. Kolakowski, "Positivist Philosophy", op.cit., p.15.

## FOOTNOTES (cont.)

## PART II: CHAPTER 5:

97. Andersen, "The State of the Monetarist Debate", op.cit., p.8. Problems of allocative detail are not considered necessary for the Monetarists since it can be assumed that "aggregate behavior can be analyzed quite independently of the behavior of individual sectors", Andersen and Carlson, "St. Louis Model Revisited", op.cit., p.305, and p.306.
98. Fisher and Sheppard, "Interrelationships between Real and Monetary Variables . . .", op.cit., p.235.
99. Teigen, "A Critical Look . . .", op.cit., pp.16-7.
100. See Klein, "An Introduction to Econometrics", op.cit., pp.180-3.
101. See Fisher and Sheppard, "Interrelationships . . .", op.cit., p.202.
102. See, e.g. M.J. Hamburger, "Indicators of Monetary Policy: The Arguments and the Evidence", American Economic Review Papers and Proceedings, 1970, pp.32-46, and R. Zecher, "Implications of Four Econometric Models for the Indicators Issue", American Economic Review Papers and Proceedings, 1970, pp.47-55.
103. Fisher and Sheppard, "Interrelationships . . .", op.cit., p.202.
104. See footnote 93.
105. See Fisher and Sheppard, ibid., pp.207-9.
106. Ibid., p.207.
107. See G. Fromm, "Survey of United States Models", in G. Fromm and L.R. Klein, The Brookings Model: Perspective and Recent Developments, 1975, p.397.
108. D. Fand, "Some Issues in Monetary Economics", FRB of St. Louis Review, Jan.1970, pp.16-9, and Fisher and Sheppard, ibid., pp.221-2.
109. Fromm and Taubman, "Policy Simulations . . .", op.cit., p.11.
110. See Ando, "Some Aspects of Stabilization Policies, the Monetarist controversy, and the MPS Model", op.cit., p.542.
111. Fromm, "Survey . . .", op.cit., p.393.
112. Fisher and Sheppard, "Interrelationships . . .", op.cit., p.220.
113. Fromm, "Survey . . .", op.cit., p.387.

## FOOTNOTES (cont.)

## PART II: CHAPTER 5:

114. Fromm, "Survey . . .", op.cit., p.408. "Despite . . . differences . . . with the exception of the FRB St. Louis Model, there is a fair amount of agreement among quarterly models. GNP-non defense government expenditure multipliers are around two after four quarters and then generally continue to rise, with slight fluctuations thereafter. Results for the annual models and L1U-HWA monthly model are consistent with this pattern", Fromm and Klein, "The NBER/NSF Model Comparison Seminar", op.cit., pp.16-7.
115. Fromm, ibid., p.408. "Most of the models . . . show such (constant dollar multipliers) reaching a peak in two or three years and then declining . . . Multipliers for the MPS model decline to negative values quite early, but not as early as the St. Louis model", Fromm and Klein, "The NBER/NSF Comparison Seminar . . .", op.cit., p.17. See also Ando, "Some Aspects of Stabilization Policies . . .", op.cit., p.562.
116. Fromm and Klein, "A Comparison of Eleven Econometric Models of the United States", op.cit., p.393.
117. Deflationary fiscal policies although qualitatively different from expansionary ones, exhibit similar responses, bearing opposite signs.
118. "The Monetarist Controversy . . .", op.cit., p.9. See also A. Ando and F.M. Modigliani, "Some Reflections on Describing Structures of Financial Sectors", in Fromm and Klein "The Brookings Model", op.cit., p.559. C. Christ also reports results from a different set of large-scale models saying that all of them "agree fairly closely on fiscal multipliers for GNP, both real and nominal . . .: the two year government purchases multiplier is put between 1.9 and 2.8 for nominal GNP, and between 1.4 and 2.4 for real GNP. All the quarterly models in Fromm and Klein . . . agree approximately with these results, except for the St. Louis model which puts the five-quarter (and later) multipliers close to zero", "Judging the Performance of Econometric Models", International Economic Review, 1975, p.65. See also Ando, "Some Aspects . . .", op.cit., pp.563-566.
119. Andersen and Carlson, "St.Louis Model Revisited", op.cit., pp.309-310.
120. Andersen and Carlson, "A Monetarist Model . . .", op.cit., p.16. See also Fromm and Klein, "The NBER/NSF Comparison Seminar", op.cit., pp.4-7 and pp.8-19.

## FOOTNOTES (cont.)

## PART II: CHAPTER 5:

121. For more recent experiments with the MPS model that contains a bigger financial sector than the CB and FRB-MIT models see A. Ando and F. Modigliani, "Some Reflections on Describing Structures of Financial Sectors", in Fromm and Klein "The Brookings Model . . .", op.cit., where they reach the conclusion that as far as the MPS model is concerned, "Monetary policies (as compared to fiscal policies) . . . will not have very substantial impacts other than to determine the level of wages and prices" (p.559). Also more recent experiments with the MPS models and other evidence, induces Modigliani to conclude that, "opting for a constant rate of growth of the nominal supply can result in a stable economy only in the absence of significant exogenous shocks . . . The clearest evidence on the importance of such shocks is provided by our postwar record with its six recessions", "The Monetarist Controversy . . .", op.cit., p.11.
122. "The pattern of the coefficients [of the total spending equation in the St. Louis model] indicates a large and rapid influence of monetary actions on total spending relative to that of fiscal actions", Andersen and Carlson, "A Monetarist Model . . .", op.cit., p.11.
123. Andersen and Carlson, "A Monetarist Model . . .", op.cit., p.20.
124. Ibid., p.20.
125. Ibid., p.20. For further results of monetary policy within the St. Louis model see L.C. Andersen and O.S. Karnosky, "The Appropriate Time Frame for Controlling Monetary Aggregates: The St. Louis Evidence", in, Controlling Monetary Aggregates, II, 1972, op.cit., esp. pp.161-177.
126. "In short, Monetarism is severely limited in its explanation of economic reality. It is, at best, only a part of the story." Klein, "Empirical Evidence on Fiscal and Monetary Models", in, Diamond, op.cit., p.41.
127. ". . . economy-wide econometric models which are concerned to evaluate the influence of the monetary sector on real economic activity are inevitably biased toward a Keynesian viewpoint". Fisher and Sheppard, "Interrelationships . . .", op.cit., p.207, and Culbertson, "Macroeconomic Theory . . .", op.cit., p.106.
128. Andersen, "The State of the Monetarist Debate", op.cit., p.4, also Fand, "Some Issues of Monetary Economics", op.cit., pp.19-20.
129. Fisher and Sheppard, ibid., p.202, and S.M. Goldfeld, "Discussion", in Fromm and Klein, "The Brookings Model", op.cit., p.355. For a critique of large-scale models on grounds of gaps between the variables specified and the series used for estimation and lack of accounting for the dynamic effects produced by changes in policies, see, P.I. Lucas, Jr., "Econometric (cont.)

## FOOTNOTES (cont.)

## PART II: CHAPTER 5:

- 129 (cont.) Policy Evaluation: A Critique", in K. Brunner and A.M. Meltzer, "The Phillips Curve and Labor Markets, 1976, pp.20-42. For a counter critique see the Comment by P.J. Gordon, ibid., pp.47-57.
130. P.S. Samuelson, "The Art and Science of Macromodels over 50 Years", in Fromm and Klein, "The Brookings Model . . .", op.cit., p.8.
131. Liu, "Discussion", in Fromm and Klein, ibid., pp.416-7, also Fromm, "Survey . . .", op.cit., p.387.
132. Samuelson, ibid., pp.8-9.
133. See Fromm and Klein, "A Comparison . . .", op.cit., p.364. See also G.R. Schink, "An Evaluation of the Predictive Abilities of a Large Scale Model: Post Sample Simulations with the Brookings Model", in Fromm and Klein, "The Brookings Model", op.cit., pp.259-260 and p.289. For an evaluation of the properties of the two approaches see Goldfeld, "Discussion" in Fromm and Klein, ibid., where he concludes that the choice between them is "an open question", p.356.
134. A. Okun, "Uses of Models for Policy Formulation", in Fromm and Klein, "The Brookings Model . . .", op.cit., p.364.
135. It is true that money is gradually acquiring a more prominent role in policy. The fact of a permanent inflation has persuaded some Fiscalists of the ineffectiveness of expansionist policies. However this ineffectiveness is due to price-rise expectations and not to the impotency of fiscal policy as such. See A. Burns, quoted in A.E. Burger and D.R. Mudd, "The FOMC in 1976: Progress Against Inflation", Federal Reserve Bank of St. Louis Review, March 1977, p.2; see also, Time, Feb.1976.
136. "Discussion" in, "25 Years after the Rediscovery of Money: what have we Learned?", American Economic Review Papers and Proceedings, 1975, p.181, and Modigliani, "The Monetarist Controversy . . .", op.cit., pp.16-17.
137. Ibid., p.177.
138. Recent reviewers conclude that, "Although there is general agreement among economists that changes in the money supply may cause certain changes in asset values, there is disagreement over the importance of money in economic activity", R.J. Rogalski and J.D. Vinsø, "Stock Returns, Money Supply and the Direction of Causality", The Journal of Finance, 1977, p.1017.
139. Ando, "Some Aspects . . .", op.cit., p.566, and pp.563-564.
140. Andersen and Carlson, "St. Louis Model Revisited", op.cit., p.309 and p.329.

## FOOTNOTES (cont.)

## PART II: CHAPTER 5:

141. "existing empirical studies (which are mutually contradictory . . .) are undermined . . . For the present, we have very little settled knowledge regarding macroeconomic knowledge", Culbertson, "Macroeconomic Theory . . .", op.cit., p.86.
142. Samuelson, "The Role of Money . . .", op.cit., p.9.
143. Heller, "Monetarism versus Fiscalism", op.cit., p.16.
144. Brainard and Cooper, "Empirical Monetary Macroeconomics . . .", op.cit., p.167.
145. Andersen, "The State of the Monetarist Debate", op.cit., pp.5-6.
146. Modigliani, "Discussion", op.cit., p.181.
147. For instance Agassi says, "... Yet the theory - the quantity theory of money as it is called - which Hume proved a priori, is still contested and still hardly tested to economists' satisfaction: is the price level fixed mainly by the amount of available money? Some economists answer yes others no", "Tautology and Testability in Economics", Philosophy of the Social Sciences, 1971, p.49.
148. "at an empirical level, a satisfactory comparative evaluation of the two theories has not emerged", Fisher and Sheppard, "Interrelationships . . .", op.cit., p.180.
149. Papers and Proceedings, 1970, pp.32-58.
150. Diamond, "Issues in Fiscal and Monetary Policy", op.cit., where Klein, for instance, claims that: "I disagree with Milton Friedman on almost all points of consequence in spite of many assertions that economists really agree on a wide variety of issues", ibid., p.42.
151. See, "The Political Economy of '76", Time, Feb.1976, p.39.
152. See the Guardian, Sep.16 and 29, 1974, June 2, 1975, May 21, 1976. For example in the Guardian of October 10, 1976, Alan Day says that the "acceptance that money matters does not imply acceptance of the prognosis of the more enthusiastic monetarists, who see a direct and very simple relationship of cause and effect between the growth in the supply of money on the one hand and the speed of inflation . . . on the other. The truth is undoubtedly more complex - and more painful".
153. See especially issues: FRB St. Louis Review, Dec.1975, Feb.1970, Apr.1970, Jan.1970, Jan.1974, Sep.1973, Jan.1972, Jan.1975, Jne.1976 (where Karnosky reestimates the Money-Price equation for the 1971-1976 period, finding evidence supporting the correlation), May 1977 and March 1977, Lloyd's Bank Review, 1970, 1975 and 1976, (cont.)

## FOOTNOTES (cont.)

## PART II: Chapter 5:

- 153 (cont.) International Economic Review 1974, 1975, 1976,  
National Westminster Review 1975, American Economic Review  
1970, 1975 and 1977.
154. O.K. Foley, "Problems and Conflicts: Economic Theory and  
Ideology", American Economic Review, 1975, p.236.

P A R T      I I I

A N    E X P L A N A T I O N    O F    C O N F L I C T

I N    P O S I T I V E    E C O N O M I C S

## P A R T   I I I

A N   E X P L A N A T I O N   O F   C O N F L I C T  
I N   P O S I T I V E   E C O N O M I C S

## O U T L I N E

The purpose of this part of the thesis is to explain the continuing persistence of the Monetary controversy. It is an attempt to understand the logic of the methodological impasse hindering the resolution of the conflict. This impasse concerns the failure of empirical testing to enable Positive economists to choose decisively between two rival theories.

The basis of the methodology of Positive economics is to continuously test theories with empirical facts. Whenever there are two conflicting theoretical schemata, the one that survives best the tide of experience is the one that should be preferred. Although there are methodological conflicts within Positive economics, there is nevertheless agreement as to what will be the tester. It is agreed that empirical, quantitative, facts will pronounce the last judgement on which is going to be the true theory. If continuous empirical assessment fails to falsify a theory - through either its predictions or its assumptions - then a Positive economist can feel confident that this theory should be the chosen one<sup>1</sup>.

Irrespective of whether one belongs to a monetarist or a fiscalist stand one always respects the validatory power of empirical

testing<sup>2</sup>. If a theory is systematically confronted with conflicting evidence one abandons the theory and looks for a better one. One is tempted to say, however, that the Monetary controversy has not been the case to prove this point. Empirical evidence has been accumulated on either side of the debate. On the one hand, Monetarists presented their findings from either single-equation tests or small-scale models, and on the other, Fiscalists rested confident on their pile of empirical facts produced from structural models. Neither, however, succeeded in persuading their opponent of either the truth of their theory or the falsity of the other's; "where Monetarism diverges from the main stream of economics, it cannot, in my opinion, stand up to the test of plausible economic analysis and the full range of empirical experience" says Samuelson<sup>3</sup>, Friedman, however, replies that, "those people who speak most loudly about the potency of fiscal policy have produced no . . . evidence. But there is a great deal of evidence which has been produced primarily by those of us who have argued for the potency of monetary policy"<sup>4</sup>. One could probably go on to cite further examples of economists in the controversy claiming that empirical evidence has favoured their own version of the truth<sup>5</sup>. However, I hope that the preceding chapters have succeeded in showing the failure of quantitative data to solve the debate.

If, therefore, empirical evidence does not prove to be a sufficient criterion for discriminating between two alternative theories, and if the controversy persists up to date without being resolved, then one should attempt to explain why Positive economics fails to fulfill its role as a valid scientific methodology able to solve a major conflict,

and still persists as the dominant methodology. How is it that such a conflict can persist within the economic orthodoxy without producing a crisis? Why do Positive economists remain convinced of the potency of Positive economics, when it is obvious that it has failed? In essence, I feel that one should answer these questions by seeking the logic behind the Monetary conflict and its persistence within the structure of Positive economics itself.

In Part I some of the ideals of Positive economics have been examined and juxtaposed to the reality of their practice. The purity of the distinctions have been shown to contradict to the vagueness and ambiguity of their definition, as well as to the reality of keeping them pure in practice. It has been further hypothesized that some of these ambiguities and contradictions arise from fundamental and unresolved problems in the theory of knowledge. In Part II the ideal of Positive economics to the effect that empirical testing should discriminate between competing theories has been examined, as it were, in action. As far as the Monetary controversy is concerned, it has been shown that its persistence proves that empirical testing does not work. It fails in its function as a criterion for the choice between conflicting theories.

In Part III I intend to analyse the structure of Positive economics and expose the deeper logic that goes on behind the persistence of the Monetary and F-Twist controversies. In doing this my purpose is to understand how two major economic conflicts are structured, and how this structure prevents their resolution.

Before doing this however, I shall review some of the methodological criticisms levelled against Positive economics. In my view the methodology literature of economics is saturated with two related criticisms: that Positive economics is (a) ideologically biased and infiltrated with value-judgements, and (b) that its scientist ideals do not correspond to the harsh reality of human society, its unpredictability and the difficulty of its quantification. By implication these criticisms are also used as arguments to explain conflict in economics.

There is no doubt that most of this criticism goes some way in understanding the methodological problems facing Positive economics. Yet methodological discussion would benefit if an understanding of the structural relations behind conflict in Positive economics was made, instead of arguing from an external point of view. Perhaps the explanation of conflict that I will attempt in this part may be considered as complementary to the above criticisms. It will mainly differ from the above approaches in that it will fuse in a structural manner the logical tensions in both the methodology and ontology of Positive economics. In doing this the necessary relations that bind the distinctions of Positive economics will be shown, and how they form the basis for the persistence of the Monetary and F-Twist controversies.

CHAPTER 6

C L A I M S   T O W A R D S   A   C R I T I Q U E

O F   P O S I T I V E   E C O N O M I C S :

A   R E V I E W

## CHAPTER 6:

CLAIMS TOWARDS A CRITIQUE OF  
POSITIVE ECONOMICS: A REVIEW

## A. THE THEORY OF VALUE AND VALUES IN THE THEORY

One of the most important assumptions, necessary for the viability of empirical testing in Positive economics, is the distinction between positive and normative propositions. This assumption is a prerequisite for the separation of what can and what can not be empirically testable<sup>1</sup>. Positive judgements, since they refer to what actually is, can be empirically tested because empirical facts represent 'what is', whereas value judgements, since they refer to what ought to be, can not be tested because 'what ought to be' is not part of the domain of empirical facts. So theories, or their predictions, are testable only if their propositions are positive judgements about empirical reality.

Neoclassical economics, according to most Positive economists from J.N. Keynes, Senior, Sidgwick to Friedman, Koopmans and Samuelson, although general and abstract, refers to actual-representative-situations and its conclusions delineate what is possible if certain conditions are fulfilled. Thus, conclusions drawn from neoclassical economic theory are assumed to satisfy the testability requirement.

Although to most Positive economists this distinction between positive and normative propositions seems to be almost a self-evident truth they, nevertheless, take great pains in their methodological obiter dicta to justify the positive character of Neoclassical economics.

This can only be explained as a reaction against the immense flow of criticisms that have tried to show the metaphysical nature of the neoclassical theory of value. In what follows I will review what I think are the representative proponents of this criticism (see also the discussion in Part I).

To begin with, one of the most important critics is perhaps Gunnar Myrdal who attempts to expose this positivist illusion of a 'wertfrei' economics by pointing out the implicit ethical assumptions of Neoclassical economics inherited by the 'natural order' and Utilitarian philosophies<sup>2</sup>. Myrdal's purpose throughout his methodological writings is to investigate the possibility of scientific economics. That is, he tries to bring into awareness the question of valuations in social theory. If economics is more prone to metaphysics than the natural sciences<sup>3</sup>, then prima facie, there is a problem as to the possibility of selecting objectively between alternative theories. Value judgements produce a "conspicuous lack of agreement among the various writers on the economic aspects of practical and political problems. . . This inability of economics to agree has almost become proverbial"<sup>4</sup>.

For the Positive economist the solution to this problem is very simple: draw your boundary line between what is to be positive and what normative and everything will fall into place; there is no reason why a Positive economist should not be 'disinterested'<sup>5</sup>: "Positive economics is in principle independent of any particular ethical position or normative judgements"<sup>6</sup>. Furthermore, disagreements

in economics are potentially resolvable, since "the difference (between policy controversies) is not a moral one but a scientific one, in principle capable of being resolved by empirical evidence"<sup>7</sup>. Myrdal, however, refers to the persistence of controversies in economics and explains it in terms of the impossibility of economics to be divested of normative implications<sup>8</sup>.

This obvious conflict of opinions between Friedman and Myrdal can be explained in terms of the different meanings attached to value judgements in each case. For the ordinary Positive economist<sup>9</sup> value judgements refer to different political ideals, systems or cultural norms, tastes, which constitute "differences about which men can ultimately only fight"<sup>10</sup>. One can always choose, in a conscious way, to formulate one's scientific hypotheses by remaining - through the means of empirical testing - detached of any political valuation. Positive economists refer to what is or could be and not to what ought to be. In contrast, for Myrdal, valuations refer to largely unconscious processes, which infiltrate the structure of Neoclassical economics. For him value judgements are not external to economics but form part of it. Economic concepts and terminology are impregnated with valuations<sup>11</sup>. The development of economics from 'natural law' doctrines and Utilitarianism and the increasingly growing empirical scope of economics, according to Myrdal, have shaped modern economic theory into a quasi metaphysical and quasi scientific construction<sup>12</sup>. The subjective and the objective are interdependent, and if the former remains latent, as in the case of Neoclassical economics, this creates contradictions leading to the generation of opposing schools of thought and consequently disagreement<sup>13</sup>.

According to Myrdal, the definition of economics in terms of ends and means will not do either, since there is no clear-cut demarcation between the two. Even more, the selection itself of a set of means constitutes a value judgement and is, therefore biased<sup>14</sup>. In fact, not only do value judgements constitute an integral part of theory but they also enter into every phase of Positive economics. The way economists have learned to look at the world, or the use of value-laden concepts induces observation and selection of facts to be conducted according to some 'principle' or 'valuation' which, if left implicit, will lead to biased inferences<sup>15</sup>. According to Streeten, "the values enter . . . the structure of theoretical thought . . . [and] are ever-present and permeate empirical analysis through and through"<sup>16</sup>.

So it seems that there is no way out for the Positive economist since, firstly, according to Myrdal, he cannot separate values from facts or ends from means, because this would be a value in itself<sup>17</sup>; and, secondly, since the selection of empirical evidence is itself value-laden. Therefore, according to this criticism, the Positive economist cannot use empirical testing as an objective criterion for discriminating between positive and normative propositions or choosing between competing theories. Consequently, the Positive economist is at an impasse, unless he accepts the metaphysical nature of his theories. But that would ask too much of him since Positive economics is, in fact, based upon the possibility of distinguishing between what is and what ought to be.

In quite a similar vein other economists, like Joan Robinson, Kenneth Boulding, Ronald Meek, Murray Rothbard, et al., have attempted to unravel the fundamentally metaphysical nature of neoclassical assumptions<sup>18</sup>. For instance Joan Robinson reaches similar conclusions when she says that "economics itself . . . has always been partly a vehicle for the ruling ideology of each period as well as partly a method of scientific investigation"<sup>19</sup>. As a "method of scientific investigation" economics should contain propositions that satisfy Popper's criterion of falsification<sup>20</sup>. As a set of ideological propositions it is, on the one hand, influenced by older schools where ideologies were more explicit, e.g. mercantilism, laissez-faire, etc.<sup>21</sup>, by the nature of the subject whereby a description of economic phenomena will involve value judgements<sup>22</sup>, and by personal valuations that enter everywhere in the economist's method<sup>23</sup>, in addition to the value-laden concepts which he has to use, e.g. equilibrium, good, welfare, etc.<sup>24</sup>. On the other hand, it is a reflection of the status quo, mirroring and justifying the structure of capitalism<sup>25</sup>.

However, if such a distinction, i.e. ideology-science, is maintained, at least as far as the "Economic Philosophy" is concerned, Joan Robinson does not deviate from standard positivist practice when she talks of ideology as "meaningless noise or . . . circular argument"<sup>26</sup> or when she says that, "The hallmark of a metaphysical proposition is that it is not capable of being tested"<sup>27</sup>. In some sense the positive and normative distinction is still maintained. Nonetheless, she does differ from the Positive economist when she accepts ideologies as structural and useful components of economic theory and does not discard them as unnecessary; in fact she says that "metaphysical statements are

not without content"<sup>28</sup> and, like Myrdal, she considers ideologies as initial visions that "provide a quarry from which hypotheses can be drawn. They do not belong to the realm of science and yet they are necessary to it. Without them we would not know what it is that we want to know".<sup>29</sup>

Thus, the positivist clear-cut distinction is replaced by the ambivalence that ideologies "do not belong to the realm of science and yet they are necessary to it".<sup>30</sup> Since metaphysical propositions cannot be tested, and since they constitute the 'world-view' of the social scientist, the Positive economist is unable to apply his criterion of empirical testing to justify his assumptions or resolve controversies about two different 'world-views'. In fact, Joan Robinson would also be at an impasse had she not provided, as she did, a way out. The way out is to make assumptions explicit, "to reveal [the] contradictions"<sup>31</sup> and since "We cannot escape from our own habits of thought . . . we go round about. We can see what we value, and try to see why".<sup>32</sup>

This alternative is very similar to Myrdal's who says that, "We must try to lay bare the specific logical errors resulting from the insertion of valuations".<sup>33</sup> In his early works Myrdal argued for an eradication of "all the valuations tacitly implied by the basic concepts with the help of logical analysis".<sup>34</sup> However, in his later works he accepts the structural role of value judgements and argues that in addition to the fact that "Explicit value premises are . . . logically required", "There is no way of studying social reality other than from the viewpoint of human ideals",<sup>35</sup> that they help to

create "a vision of what the essential facts and the causal relations between them are"<sup>37</sup>.

Thus, like Joan Robinson, Myrdal shows that, since valuations are present everywhere in economic theory and methodology and since social phenomena are by themselves conducive to ideology formation, there is no way out but to try to make the latent valuations explicit and accept them as an integral part of theory which gives it meaning and content. "The only way of defending 'objectivity' in research is to work with explicit, often alternative, specific value premises"<sup>38</sup>.

In reviewing several other critics of Positive economics, who follow similar lines as Myrdal's and Robinson's, one observes the recurrence of the same argument, when discussion is turned towards finding a remedy for the problem of the indispensability of value judgements. For example Macfie says that, "all personal views have individual bases, and so are prejudiced. The only way then to reach towards truth . . . is to understand all relevant prejudices"<sup>39</sup>. Also Rothbard claims that it is "incumbent upon economists to present a coherent and supported ethical system or forever hold their valuations and political peace"<sup>40</sup>. Meek also declares that "'ideology' will always be with us . . . Surely, to a limited but significant extent, reason can help - and not least in making us conscious . . . of our own ideologies"<sup>41</sup>. Finally, Little maintains that,

Welfare economics and ethics cannot, then, be separated. They are inseparable because the welfare terminology is a value terminology . . . If the value premises are made explicit, and are not hidden, the result will be informative and interesting - and cannot be misleading.<sup>42</sup>

The regularity of such a solution to the problem of value judgements in economics I think, needs some explanation. As soon as the economist accepts the necessarily ideological character of value theory he is left with the task of justifying his/her own assumptions about the nature of economics. If, on the one hand, he/she accepts that what he/she says is objective then he/she falls against his/her own criticism to the effect that valuations enter everywhere in every theory and methodology. If, on the other hand, he/she acknowledges the possibility of his/her critique of ideology in economics being itself ideological, then he/she must try to justify his/her choice of the particular ideology used. However, since there are no criteria left for him/her since value judgements enter everywhere, he/she has to find the way out of the vicious circle by stating that if valuations are made explicit and act as a source of theory, then everything would be less contradictory and certainly more relevant. Nonetheless, the economist is still left with the perennial quest of having to find objective, detached, criteria for selecting between alternative explicit and relevant ideologies.

Both Robinson and Myrdal, in fact, are aware of this problem. Both, however, do very little to find a satisfactory solution. For instance Robinson says that, "The objectivity of science arises, not because the individual is impartial, but because many individuals are continually testing each other's theories"<sup>43</sup>, or that, "logic will dissolve . . . [an] ideological proposition into a completely meaningless noise"<sup>44</sup>, or, "Adopting Professor Popper's criterion for propositions that belong to the empirical sciences, that they are capable of being falsified by evidence"<sup>45</sup>. Thus, the impression that one gets in

reading Joan Robinson's remarks regarding criteria for the possibility of sorting out "this mixture of ideology and science"<sup>46</sup> is an acceptance of metaphysics, along with an effort for community testing, logic and falsifiability. However, it seems that Joan Robinson is asking too much from the novice economist when she tells him/her that, on the one hand, everything is metaphysical and yet, on the other, something ought not to be metaphysical. Firstly, if each individual is biased by ideology, either implicitly or explicitly, then there will be a tendency for fruitless conflict rather than for constructive testing of each other's ideologies. To test a theory constructively one has to be detached from one's ideological position, a task which, according to Robinson's idea of interdependence between theory and ideology, cannot be fulfilled. Secondly, logic cannot be used as a tool for dissolving metaphysical propositions since, on the one hand, we would fall in the same position as that of the Positive economist, who by the application of logic, distinguishes between positive and normative and between synthetic and analytic statements, and since, on the other hand, if each individual is permanently tainted with ideology he will always tend, openly or tacitly, to logically rationalise and justify his position. Thirdly, the falsifiability criterion (at least as used by Popper) clearly cannot function, since it implies that there is an objective tester, i.e. empirical facts. However, for Joan Robinson empirical facts cannot be objective, since she has argued that empirical facts can be chosen so as to justify any ideology. In fact the selection of one set of evidence over another is itself a value judgement<sup>47</sup>.

Similarly Myrdal, after having pleaded for the unravelling of hidden valuations and for the full acceptance of them, formulates his criteria of objectivity by saying that, "Logically, the only distinction that is scientifically valid is the one between more relevant factors and less relevant ones"<sup>48</sup>. In addition, according to Streeten's report of Myrdal's methodological position, "The logical crux of science (is) the continual encounter - sometimes constructive, sometimes destructive - between the a priori and the a posteriori, between vision and experience"<sup>49</sup>. Thus, Myrdal's criteria for choosing between explicit, alternative value premises are: (1) relevance and usefulness<sup>50</sup>, and (2) dialectical testing between theory and fact. As far as the first criterion is concerned, one is in difficulty in trying to define relevance or usefulness. Since value premises are part of the structure of economic theory, relevance will be defined according to each different and alternative value premise. For example, the priority of studying the pattern of income distribution may be relevant to Myrdal who believes that a particular pattern of income distribution affects economic variables such as capital and labour shares, prices, savings, etc., whereas to people like Friedman and Samuelson, for instance, in compliance with traditional neoclassical theory, income distribution is determined by the formation of prices according to the decision taking of producers and consumers, and therefore its study is secondary to that of price formation and consumption. Another example can be taken from the Monetary controversy, where one sees that, for the Fiscalists, the belief for a detailed and structural analysis of the economic processes will render relevant the analytical study of individual, disaggregated sectors, whereas, for the Monetarists,

a belief in simple hypothesis testing will render relevant the study of aggregated variables<sup>51</sup>. It appears, therefore, from the above examples that relevance and usefulness are very much a function of the type of questions one asks and ultimately of ideologies<sup>52</sup>. And even if the latter were made explicit we could not discriminate between alternative sets of ideologies according to the criterion of relevance, because it is assumed that ideologies and relevance are interdependent. In effect, there must be a third criterion which will enable us to choose between alternative value-laden theories. If we accept Myrdal's second criterion of "continuous testing" between theory and fact, we must assume that, as in the case of Robinson, at least, at a point of time in this dialectical process, there must be a moment of independence between "vision" and "experience". In other words, irrespective of whether it is the theory that tests the facts or the facts that test the theory, there must be either a set of theories or a set of facts that can be, firstly, independent of each other and, secondly, objective. In this sense, therefore, Myrdal's criteria are nothing more than a desperate cry or an act of faith, when he has already said that facts and theories are, in fact, interdependent and that there is no way of separating between the two - unlike the Positive economist who, epistemologically, accepts such separation<sup>53</sup>. For example, Myrdal says that, "Facts come to mean something only as ascertained and organized in the frame of a theory . . . [and] the truth about society is therefore always a theory: a vision of what the essential facts and the causal relations between them are"<sup>54</sup>, or in a different context, the "structural interdependence of valuations and facts is presented as a necessary condition of all . . . theory and research"<sup>55</sup>. Like Robinson, Myrdal

accepts the implicit proposition that theories and facts are interdependent and yet sometimes they are not. Clearly, one has to provide 'objective' criteria to show how one knows when a theory is independent of fact and vice versa. However, this is a position which is denied to Myrdal, because according to his methodology criteria will always imply value judgements. In fact, the position that is implicit in both Robinson's and Myrdal's epistemological claims, leads necessarily to relativism<sup>56</sup>.

Myrdal does recognize this 'danger', perhaps more than Robinson, when he says,

But is not the proposition that politics ought to be rational . . . and that economists ought to support this endeavour itself a normative principle? And is it not arbitrary at that? . . . The answer to this question is that the possibility of scientific endeavour depends upon the tacit assumption that rational is desirable.<sup>57</sup>

However, it seems that the desirability of rational, or objective, argumentation or the "possibility" of science are beliefs and values well entrenched in our system of thought and, therefore, as such do not constitute 'objective', 'rational' or 'experimental' processes, but they are rather acts of faith. After all, as Myrdal says, the "tacit assumption must be made explicit and then if it is why not sacrifice 'truth' to higher-values"<sup>58</sup>.

By critically reviewing some of the criticisms levelled against Positive economics, I do not purport to either justify Positive economics or to show that the criticisms in themselves are wrong.

What I do want to show, however, is that, while most of the criticisms referring to the role of ideology in economics are in themselves correct, nevertheless in the alternative offered they create an inherent contradiction, i.e. they accept the simultaneous existence of ideology and science. However, to say that there is a logical contradiction does not mean that the alternatives are irrational or fruitless. They themselves may serve, according to Robinson and Myrdal, as structural components of economic theory, with a meaningful role. As Streeten puts it, "The attempt to save the theory from self-contradiction succeeds only through an arbitrary step into metaphysics"<sup>59</sup>.

Finally, in this section, I will make two additional observations in reference to this category of criticism. The one pertains to the comparison between the critics' alternative and Positive economics, and the other to the adequacy of a 'value-laden economics' criticism as an explanation of the persistence of economic controversies.

As far as the first observation is concerned Myrdal claims that in Neoclassical economics valuations are latent and permeate the concepts and terminology everywhere, whereas his alternative is to be self-critical and bring out in the open all the metaphysical implications found in a theory; "we must try to lay bare the specific logical errors resulting from . . . valuations"<sup>60</sup>.

However, it seems somehow puzzling, and somewhat presumptuous, to say that one is self-critical and makes one's ideology explicit while

the others are not - moreover to claim self-criticism for oneself is quite uncritical of the claim of self-criticism. True enough Positive economists usually do not tend to recognize the existence of value judgements in neoclassical theory. Nonetheless, this is not evidence enough for claiming that valuations are left implicit by Positive economists. When Myrdal or Robinson claim for making ideologies explicit, in fact they assume that there are ideologies, whereas for the Positive economist it is not at all obvious that there exists unconscious ideological bias that cannot be purged by empirical testing. Moreover, Positive economists from J.N. Keynes to Robbins and Friedman have made their assumptions (not as ideologies) explicit and tried to defend them rationally. For instance, Friedman says, "In seeking to make a science as "objective" as possible, our aim should be to formulate the rules explicitly in so far as possible"<sup>61</sup>. I do not think that it is the case that Positive economists are 'naive' or are not 'self-critical' necessarily, but it seems to me that they take an epistemological stand which offers the possibility of separating between positive and value judgements. After all, Myrdal's and Robinson's position of accepting the interdependence of positive and value judgements is itself an epistemological datum; as Myrdal admits: "The actual choice of viewpoint and categories will, of course, depend, in the last resort, on the underlying epistemological approach"<sup>62</sup>.

Thus, it is not necessary, i.e. stemming necessarily from their epistemology, that Positive economists cannot question their assumptions. Although it may be true that most of the time

self-criticism is not a virtue characteristic of Positive economists, yet it cannot be claimed that Positive economists are necessarily not self-critical - but rather that they belong to an epistemology that defines ideology differently. Accordingly, I do not think that it is fruitful to criticize Positive economists from the point of view of a different epistemological paradigm as this would not show Positive economists their implicit contradictions, but rather convince them that the critic is speaking from a different and most of the time 'nonsensical' point of view.

This kind of criticism seeks to find defects in the process of theory selection in Positive economics, that may be considered as causes of persistent controversy. However, Positive economics is a paradigm that contains a self-contained coherent logic. By finding defects that arise mainly from viewing Positive economics through the eyes of an alternative paradigm, one does not seek to understand the logic of Positive economics, but perhaps to demolish it and replace it. I believe that in offering an explanation of conflict one should analyse the structure of Positive economics and expose all the logical connections therein that lead to such conflict.

For example, if we were to apply the 'value-laden economics' criticism to explain the persistence of the Monetary controversy, it would seem that there are two factors missing. Firstly, assuming that the Monetarists and the Fiscalists have different ideologies and also assuming that these ideologies are latent, then would there be a resolution of the conflict if the latent ideologies were made explicit?

If we take Myrdal's early position, i.e. exorcising valuations by making them explicit, it follows that by making the ideologies overt we are able to logically analyse them, expose their contradictions and meaninglessness and thus discard them. This, consequently, would leave us with pure logic and fact, in which case the controversy would be resolved. However, one cannot assume that facts and theories are interdependent and that values are needed to select facts and yet also assume that by making values explicit we are divested of them. It seems that even if ideologies were exposed the controversy would still persist, since there could never be a complete independence between vision and fact. Explicit or implicit values would always enter in the logical (analytical) concepts and in the selection of facts and the debate would go on. If, on the other hand, we take Myrdal's later epistemological position, i.e. accept valuations as structural components and useful parts of theory, we would still have two 'ideologies', say Monetarism (*laissez-faire*) and Fiscalism (interventionism), completely different, which moreover, if they were made explicit, would reveal their dissonance even louder and clearer, thus generating a greater gap between the two opposing factions. If Samuelson or Friedman, for example, would see that they are ideologically biased - an unlikely event - they would find it impossible either to accept this bias as useful, since they are Positive economists and as such do not have any use for ideologies, or to discard it, since this would imply discarding their 'identities' as Monetarists and Fiscalists. Prima facie, if there are two diametrically opposed ideologies, and they are made explicit, their strength grows rather than diminishes. Secondly, an intra-paradigmatic conflict, such as

the Monetary controversy, cannot be explained by only appealing to the two completely different ideologies as this would imply rather an inter-paradigmatic conflict, such as for example between Marxism and Neoclassical economics. To be able to explain a conflict within a paradigm one would need something more than ideological differences. One would need to find a contradiction or a conflicting duality embedded in the structure of the paradigm itself, which would be logical (structural) rather than only ideological. This logical contradiction, as we shall see in the following chapter, would be such as to bind the structure of the paradigm, despite inner conflict, in a coherent, self-contained form.

If, therefore, a logical contradiction runs through the construction of Positive economics, and is so situated as to form complementary (binary) logical opposites, then a tension must exist which allows and in fact nurtures the development of opposing schools of thought. This tension must be so structured as to render the resolution of the conflict impossible, unless a new paradigm with different epistemological and theoretical conceptions and different logic is introduced.

To summarize: the greatest part in the effort to criticize Positive economics focuses on the role of value-judgements and ideology in theory formation and validation in economics. As we have seen this criticism is important because, if correct, it destroys the whole positivist mechanism of validation. In this sense the role of ideology may be construed as important in explaining the persistence

of the Monetary controversy. If, for example, ideologies are intricately woven with theories and if, in addition, valuations form a necessary part of empirical evidence and testing, then the controversy will persist, since there are no criteria to render any theory valid. The two opposing factions will stand by their ideologies and valuations and the controversy will continue and, ultimately, reach an impasse.

In applying the ideology-explanation to the Monetary controversy Culbertson, for instance, claims that,

the hidden political content of professional work may lead theory and research to be cast into a form that does not lead to resolution of conflicts and cumulative refinement of knowledge, but rather to continuing conflict between partisan groups, governed by their political positions or ideologies,

and concludes that, "the driving force in the whole controversy has been ideological"<sup>63</sup>. Consequently, if Positive economics is consistent with its rules, the Monetary controversy, as it is a major one, will become the basis for a methodological crisis in Positive economics<sup>64</sup>.

The paradox in the case of the Monetary controversy, however, is that although there is wide agreement as to the power of empirical testing to discriminate objectively between competing theories, and that although empirical testing has been shown inadequate to resolve the controversy, yet economists from both sides of the debate continue to use empirical testing as the sole criterion of objectivity persistently. It seems as if there is a binding force keeping economists tied to the

positivist obligation to use empirical testing as a valid criterion and to feel justified in doing this. Consequently, one cannot, at least sufficiently, argue that the Monetary controversy persists because the belief in empirical testing constitutes a value judgement or ideology. Although this forms part of the explanation it cannot fully account for the continuing insistence of Positive economists to use empirical testing when it is obvious that it has not fulfilled its role as a validational criterion, at least as far as the Monetary controversy is concerned. Obviously, this complex phenomenon of a controversy persisting through the history of economics calls for a fuller explanation. One that would delineate the structure within which ideas about ideology and methodology are intertwined.

Another explanation that can be given for the paradox (belief-failure-persistence) is in terms of 'paradigms', 'anomalies' and 'crises' in a Kuhnian, modified by Ward, sense. According to this explanation, as economists are members of the same paradigm they use empirical testing as part of their training and their acquired 'weltanschauung'. The question whether empirical testing has been shown to be a success or a failure (an anomaly) is irrelevant to the Positive economist who is attached to the principle on the grounds of (unconscious) faith to the paradigm rather than pure rationality or objectivity. In other words, crisis is averted in Positive economics by sweeping, as it were, the failure of empirical testing under the carpet of the profession and the paradigm.

However, for this explanation to apply one must assign to Positive economics the status of a paradigm. The Kuhnian interpretation of scientific development in terms of paradigms and anomalies, though perhaps more appropriate to physics, cannot be straightforwardly applied to economics. This is because of the relationship between economics and the social structure. A change in the social structure brings about a change in both economist and economy, whereas it brings about a change only in physicist and not in matter. Thus, for example, class antagonism permits the parallel development of conflicting economic schools (e.g. Marxism and Marginalism). The status of a paradigm is assigned to that school favoured by the dominant class, with no necessary validity accompanying it according to the mechanism of the Kuhnian theory. The validity it has reflects the interests of the class (forming part of the social structure) in power. Despite these difficulties in applying the Kuhnian interpretations in a straight forward manner, however, hints and ideas can be borrowed that can be used to explain crisis, disagreement and development in economics<sup>65</sup>.

The two above-mentioned explanations (ideology and paradigm) are indeed important, and perhaps, complementary interpretations of conflict in economics. However, what is of interest in this thesis is the logic that goes behind the aforementioned paradox. One can hardly expect a Positive economist to accept that he/she acts on faith or ideology<sup>66</sup>. In order to provide an explanation that accounts for such a logic, the structure of Positive economics has to be logically decomposed and reconstituted (always from the point of view of the

Positive economist). The implicit rationalization made by the Positive economist to reconcile the failure of empirical testing and his/her insistence in using it, is to be found here. However, before proceeding in this examination, another set of criticisms will be reviewed.

#### B. THE 'SCIENTISM' OF THE SCIENCE OF ECONOMICS

In addition to the criticisms that economics is ideologically biased, there have been numerous other criticisms levelled against the methodology of Positive economics. One can separate these criticisms into two broad categories. The first one refers to the lack of precision and experimentation in the social sciences, the scope of economics, i.e. the openness of the social system, the need for social integration and the consequent use of ceteris paribus clauses. The second category refers to the gap between theory and reality and to the consequent irrelevance of neoclassical theory. Needless to say that there have been other types of criticism, but I believe these are the most crucial ones, and the ones that have taken the greatest part of the literature. Obviously these two categories are interrelated - for instance lack of experimentation implies an open system which implies need for integration of social phenomena, which in turn touches upon the question of theoretical abstraction, delimitation and relevance.

Regarding the second category of criticisms, i.e. the gap between economic theory and reality, many of the issues involved have already been discussed in relation to the F-Twist controversy in Part I of the thesis. The old question of whether the static analysis of

equilibrium is relevant to the economic problems facing modern society has been the favourite criticism among historically and institutionally oriented economists<sup>67</sup>. However, with the advance and popularity of empirical investigation, equilibrium analysis has somewhat lost its empirically true value and has become a framework, which has only an "as if" value<sup>68</sup>. Accordingly, the issue of the relevance of neoclassical theory has stayed within the broader context of the F-Twist controversy.

Regarding the first category, i.e. experimentation in economics, Positive economists believe that although there is a difference between natural and social phenomena this difference is not of a kind but only of degree. The acquisition of objectivity for Positive economists is only a matter of refining techniques to overcome the obstacles put by social phenomena and measure precisely. In a sense, it is believed that differences in phenomena are countered by a methodology, i.e. Positivism, that offers the same criteria of validation for both physics and economics.

Against this there have been criticisms that go as far back as the rise of English and Austrian Marginalism and its opposition German Historicism. The criticisms and the consequent debate go on even today<sup>69</sup>. A fundamental argument challenging the positivist contention of the possibility of controlled experimentation in economics refers to the difficulty of differentiating between the observer and the observed. According to this argument whereas matter does not have consciousness and thus can be controlled, man, the subject of

economics, does and is, therefore, uncontrollable and unpredictable. Moreover, not only is the distinction between the observer and the observed blurred in the case of economics, but in fact the observer is also the observed. Two representative proponents of this argument are Hayek<sup>70</sup> and Knight<sup>71</sup>. Although neither of them belong to the Historical or Institutionalist school, they nevertheless share a great deal of the latter's criticism of Positive economics. Both Hayek and Knight hold that the phenomena of society are not conducive to experimentation and that, therefore, a different methodology - such as introspection - is needed<sup>72</sup>. However, the Institutionalists or Historicists differ from Hayek and Knight in that they accept a wholistic picture of society whereas Knight and, especially, Hayek reject the concept of whole and embrace the concept of the individual.

There are two reasons advanced for showing the impossibility for economics to emulate the methodology of the experimental sciences. The first one relates to the openness of the economic system, and the second one relates to, what can be generally called, the consciousness of the subject of economics. These two reasons, according to the critics, constitute an important barrier to the application of experimental methods that could render feasible the accumulation of objective and exact empirical evidence.

As far as the first reason is concerned, it is argued that the neoclassical definition of economic reality in terms of scarce means and multiple ends, along with the positivist insistence to focus only on the quantifiable aspects of society, imposes an artificial limit

on the scope of the science and offers a distorted view of economic action<sup>73</sup>. In essence it is argued that a theoretical, and therefore an ad hoc, delimitation of economics, with the help of ceteris paribus clauses, cannot be expected to be true of actual social reality, whereby the interdependency of all human factors prohibits the possibility of controlled experimentation<sup>74</sup>. While the natural sciences also exhibit similar difficulties<sup>75</sup>, it can still be maintained that the impact of exogenous influences on the phenomenon under investigation is, in principle, relatively measurable and thus controlled. A theoretical ceteris paribus could be true in Neoclassical economics if a conceptual exogeneity of external factors is assumed<sup>76</sup>. However, for the 'real' world, either the effect of every exogenous factor must be measured (something which cannot be comfortably claimed for economics), or the study of society should be conducted on a more integrated and, therefore, more qualitative basis<sup>77</sup>.

Thus, it is claimed, because of the nature of economic phenomena experimentation or quantification can never acquire the 'hardness' of the natural sciences. If the subject of economics is man then, although theoretically we could speak of economic man, it is virtually impossible to do so in empirical terms<sup>78</sup>. If the openness of the social system is accepted then economics should go beyond quantification and study the total structure of society, in which case it would mean that objectivity would have to take a different meaning than the one implied by experimentation<sup>79</sup>. The problem here, as in the case of the previous section, is that once quantification, and therefore fixed criteria of objectivity, are rejected, validation becomes

a shaky construct based upon, for instance, "introspection"<sup>80</sup>, "interdependency"<sup>81</sup>, or, once again, making valuations explicit and study in a qualitative manner the institutions that induce them<sup>82</sup>. However, before I discuss these alternatives I will consider the second reason offered against the use of positivist methods in the social sciences.

The phenomena of physics concern inanimate objects the behaviour of which can be observed from a 'distance', i.e. in an objective manner<sup>83</sup>. In contrast the phenomena of economics (human behaviour) concern objects that are characterized by consciousness and purposive action. This feature of economic phenomena raises two problems: firstly, it is very difficult to control human behaviour - due to the variations in purpose and consciousness - and therefore experiment with it<sup>84</sup>, and secondly, the observers themselves are, (a) persons with consciousness<sup>85</sup>, and (b) parts of the phenomenon under observation. According to Knight,

all such knowledge (of social) phenomena] is inseparable from (a) self-knowledge of the knower and (b) knowledge of other knowers and their knowledge, or of their 'minds', and hence on the nature and conditions of knowing and thinking as such.<sup>86</sup>

The criterion of the independence between knower and known forms the cornerstone of positivist epistemology. If this criterion is perceived as invalid when applied to the social sciences, the prospective of a Positive economics is greatly weakened. This criterion is also rejected by Hayek who claims that the categories and facts

of economics are part of the way we classify things<sup>87</sup>. There is no external knowledge of objects and phenomena but only internal knowledge of our own classifications. According to Hayek,

all mental phenomena [including economics], sense perceptions and images as well as the more abstract "concepts" and "ideas" must be regarded as acts of classification performed by the brain . . . the qualities which we perceive are not properties of the objects but ways in which . . . [we] have learnt to group or classify . . .<sup>88</sup>

Thus, according to the above, there is no objective, i.e. external, way to resolve, say, the Monetary debate. The accumulation of empirical evidence does not help because empirical evidence is, firstly, produced by objects the attitudes of which are perceived by the observer through his/her a priori classifications, and secondly, dependent on theory, since its selection is determined by our acquired preconceptions, parts of which form the theory in question. For example, according to this argument, the pattern of the money supply lagging always behind income is a product not of the structure of the economy, but a product of the structure of our mind. Thus, a different structure of mind would produce a different structure of the economy and thus a different macroeconomic theory, e.g. income lagging behind money. Our external observations and experiments are, therefore, subjective and can not be used as a criterion for choosing between competing theories. In other words, the evidence from either single-equation tests or large scale models is not independent of the models themselves and is consequently inappropriate for appraising the theories behind them.

The question whether experimentation is possible in economics has been the focus of many methodological discussions. In my judgement, the central issue revolves around the definition of 'fact', 'evidence' or 'experience'<sup>89</sup>. Once a Positive economist is clear about the meaning of 'fact', i.e. quantified attributes of observed behaviour, then it follows that it is legitimate to consider the methodology of the social sciences equivalent to the methodology of the physical sciences. Comparison of empirical facts with predictions is considered sufficient for theory appraisal. Moreover, although Positive economists would admit the openness of the system, they would still claim that experimentation in economics does not involve, in principle, more problems than physics<sup>90</sup>. It is believed that the development of quantification would limit the scope of economics and provide a definite domain<sup>91</sup>. On the other hand, once it is accepted that quantification is not possible in economics, due to the qualitative character of economic experience, then facts become more fluid and lose their assymetrical power with respect to theory validation. In effect, experimentation seems impossible because of the indeterminacy and interdependency of economic experiences, and because facts are considered to be part of theory<sup>92</sup>.

In a sense, the problem goes back to the role of value-judgements and valuations. Since Positive economics does not recognize this fusion between 'classifications' (valuations) and experience as valid, it can legitimately claim validity for quantification and experimentation. If, on the other hand, this fusion is accepted and empirical facts lose their objectivity, then a different epistemology is called for. Ultimately, we would have different

epistemologies basing their claims on different views about the nature of reality (ontology). For the Positive economist reality is such that it can be quantified and externalized, whereas for people like Hayek and Knight economic reality is not external to the observer but forms part of his/her "classifications", thus losing its objective externality. It follows that Hayek and Knight would reject experimentation and call for "introspection". We have therefore two epistemologies based on two ontologies. If, however, the function of an epistemology (theory of knowledge) is to support an ontology (theory of reality)<sup>93</sup> and if, on the other hand, the epistemology itself is based on an ontology, then there is a need for another epistemology which would support this ontology. The consequence is, as Hollis and Nell observe, an infinite regress<sup>94</sup>. It follows that the task of choosing between any two alternative epistemologies is not easy, and unless a justification of the initial epistemology is given, it may lead to an impasse. In what follows I shall review Hayek's and Knight's justification of their epistemologies.

Hayek claims that "wholes" cannot be perceived and therefore the individual is the proper object of inquiry<sup>95</sup>. Since the structure of the mind of all individuals is similar<sup>96</sup>, he proposes that economic knowledge becomes possible only through introspection<sup>97</sup>. This position follows from the fact that "Our procedure is based on the experience that other people as a rule . . . classify their sense impressions as we do"<sup>98</sup>. Accordingly, if there is a common structure underlying our thought and if the subjective is linked with the

objective, then inward self-knowledge is the only way to study social phenomena<sup>99</sup>. However, if we think of introspection as a viable methodology, how can a conflict of ideas, such as in the Monetary controversy, be resolved? If we introspect the only thing we will be able to see within us is our own ideas and values which will always justify our own position. If the other faction does the same then we will need a third faction which will (introspectively?) decide for the superiority of one position over the other. Clearly the acceptance of a third faction acting as an objective (external) criterion of validation is denied to Hayek who claims that everything is known through a priori classifications. The fundamental contradiction in Hayek's position is that the idea of introspection in itself implies previous classifications (known to us through other introspections?). In other words, if our knowledge is determined by a priori classifications of categories, then introspection of our internal knowledge needs a further introspection to be justified, since it is a category of our a priori classifications. Finally, Hayek's ontological statement about the existence of a common mind structure, is itself a statement which either has to be proved empirically or introspectively. The first position is rejected by Hayek himself and the second creates an infinite regress.

Knight also starts with the same assumption about the priority of the individual over the whole in social investigation<sup>100</sup>, and the interdependency between the knower and the known<sup>101</sup>. Although he also proposes introspection as a suitable method for economics, he

also adds as a criterion of objectivity the idea of a social critical consensus. Knight bases his logic of theory validation on a scientific ethic. Firstly, introspection will reveal to us theories about society<sup>102</sup>, and secondly, these theories will be criticized by "competent observers"<sup>103</sup>. Obviously problems such as the ones discussed in the previous section will arise. For instance, what are the criteria of competence or what are the criteria with which we will be able to achieve consensus in the first place, and in the second, why should competent observers be objectively critical? As Winch argues, making an external check upon one's actions or theories implies the establishment of a standard. However, standards and rules have their basis in society, and in society there may be different ideologies implying different and irreconcilable standards<sup>104</sup>. In fact Knight is aware of these problems when he says that,

a consensus regarding truth is itself by no means a 'mere' (undisputed) fact. It rests upon value judgements as to both competence and the moral reliability of observers and reporters.<sup>105</sup>

Scientific truth then according to Knight, is a value judgement, and theory appraisal depends upon criticism from competent observers: "truth is a value, established by criticism"<sup>106</sup>. One might argue, however, that criticism may stem from different ideological perspectives, to which Knight replies that, "Without a sense of honour (as well as special competence) among scientists . . . there could be no science"<sup>107</sup>, and that, "objectivity in this field [economics] is established only by verification by other observers, which depends on inter-communication and also on the competence and integrity of the observers"<sup>108</sup>. Thus Knight's criteria of theory validation are:

honesty-integrity, competence and inter-communication. Apart from the problems of divergence of ideologies, conflict of opinion, subjective bias, there is also the problem of applying the above criteria in theory selection, say, in the Monetary controversy. Surely, no one could argue against the integrity, competence or inter-communication of either the Monetarists or the Fiscalists. Although there is no doubt that members of both schools fulfil all the above criteria one can see little progress achieved in resolving the controversy. Moreover, one would need to prove one's honesty and competence before one could use these qualities to select between alternative schools. Which means that a set of fixed criteria have to be established for the determination of honesty and competence. For instance, as far as Marxism is concerned, the criterion of competence would not be fulfilled, when competence is defined according to the standards of Positive economics, and vice versa. Also it is hard to see proper scientific inter-communication in the social sciences - in the sense Knight means it - where fixed ideological and a priori methodological perspectives keep opposing factions in constant distance.

Knight's and Hayek's observations as to the nature of economic reality may be correct insofar as they are used as a critique of Positive economics. But once the critique is turned into assumptions from which a new social ontology and methodology flow, one is left with alternatives that carry the contradictions found in Positive economics, only in a reverse manner. In other words, when subjective

states are recognized as part of scientific procedure and when the qualitative character of social phenomena is ascertained, then Positivism, which emphasizes objectivity and quantification, falls as a scientific methodology. However, an alternative methodology should not take subjectivity and qualitativeness - used as a critique of Positive economics - and turn them into premises from which the alternatives are formed. This follows from the fact that once objectivity, i.e. independence of observation and quantification, and thus fixed criteria of validation, are rejected, then one is at difficulty in avoiding relativism. In the case of Hayek and Knight, the premise of subjectivity leads necessarily to introspection, which as a scientific criterion is hard to justify. Perhaps what is needed is a theory of social action which is not merely the opposite of Positive economics, but which is based on a completely different paradigm incorporating different ontological and epistemological assumptions.

In this chapter I have discussed critiques of Positive economics and factors that can be used to explain irresolution of controversy in economics. These are: value judgements and ideology, subjectivity in observation and the impossibility of experimentation. As we have seen these factors may be taken as sufficient but not necessary explanations. They are not necessary because they largely refer to the defects and shortcomings of economic theory and method and not to the inherent logic that propagates the continuation of conflict in economics. If it is assumed that the potential development of techniques and methods could overcome defects such as subjectivity,

values and consciousness in both observer and observed, and render economics objective, then controversy in economics would be resolvable since the obstacles would have been removed. Following section C which will deal with a more systematic explanation of conflict in economics given by Krupp, I shall attempt to put forward a necessary explanation of the persistence of controversy, and particularly of the Monetary controversy. This explanation will account for the logical impossibility of resolution in economic disagreements, irrespective of the potential development of techniques and methods.

#### C. KRUPP'S EXPLANATION OF CONTROVERSY

Once the validity of empirical facts (as reflecting the 'real' and 'true' structure of experience) is unquestioned, and once empirical facts, however, have been shown incapable of resolving theoretical disagreement and unravelling the truth, then an explanation is needed to account for this inconsistency. Sherman Krupp attempts to do this by delineating four areas of disagreement and five factors that explain it<sup>109</sup>. The possible areas of disagreement are:

- (1) assumptions and axioms of a theory, (2) implications,
- (3) applicability, and (4) value-judgements<sup>110</sup>. These areas may be easily recognized in the case of the Monetary controversy. Firstly, there is disagreement concerning the assumptions of price flexibility and full employment, secondly, although both factions accept the common IS-LM framework, they deduce different implications about the transmission mechanism of monetary impulses, i.e. Fiscalists accept an inverse relationship between the rate of money growth and interest

rates, whereas Monetarists accept only the long-run direct relationship, thirdly, there is disagreement concerning the boundaries of the testing scope of the theories, i.e. Fiscalists prefer disaggregated and descriptive observations, whereas Monetarists prefer simple and aggregated observations, and fourthly, there is disagreement as to the desirability of fiscal or monetary policy, the objectives of price stability or full employment, and the degree of government intervention.

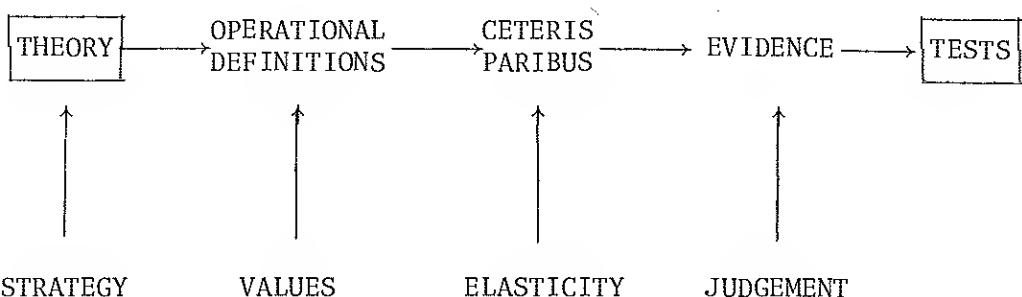
For the Positive economist, however, these areas of disagreement are potentially resolvable with empirical evidence. Yet, as Krupp points out, "Perhaps the most important observations that can be made about . . . theoretical controversy . . . is that these arguments cannot be resolved on strictly empirical grounds"<sup>111</sup>. This fundamental characteristic of controversy is also representative of the Monetary dispute. Empirical evidence has contributed very little towards the resolution of the debate. Empirical tests have not succeeded in persuading either faction.

Why is this happening? Krupp offers an array of factors that are supposed to explain this phenomenon. The first one relates to the choice of strategy, i.e. the choice of perspective from which the economist will look at the world: "the framework chosen focuses the direction of a theory's development"<sup>112</sup>. According to Krupp, the choice of strategy cannot be justified with empirical facts as its preference is based upon a priori preconceptions<sup>113</sup>. The second factor concerns the connection of theory with observable reality:

"operational definitions can always be brought into question" and therefore "theories are resistant to simple disconfirmation by the usual application of testing procedures"<sup>114</sup>. The third factor refers to the use of ceteris paribus conditionals. If the theory is elastic, i.e. too broad and general, then it easily survives empirical testing<sup>115</sup>. The fourth factor concerns the presence of judgements in the selection of evidence<sup>116</sup>. And the fifth factor relates to the existence of values in the formation and choice of definitions and sets of evidence: "Differences in values affect many questions that seem to be empirically determinable"<sup>117</sup>.

The picture that Krupp presents in relation to the difficulties in resolving controversies in economics is depicted in the diagram in Figure V. The subject in a controversy is theory (or two alternative theories) and the objective is testing it. The steps that are required for theory to reach its objective, i.e. validation, are operational definitions, ceteris paribus clauses and the selection of evidence. However, in all these steps there is an element involved which interferes with the smooth flow of the procedure. Starting even with the subject, i.e. theory, its shape is determined by preconceptions; then in connecting the subject with the real world, values enter in the definition required to do this. The difficulty of testing the theory increases when conditionals render the assumptions of the theory too broad, and finally, the selection of alternative sets of evidence requires "delicate judgement" which further distracts the procedure.

FIGURE V: SCIENTIFIC PROCEDURE AND KRUPP'S EXPLANATION OF CONTROVERSY



Most of these factors can be used to explain the persistence of the Monetary controversy. For example, the two different strategies used, one accepting the smoothness and continuity of the competitive market, while the other accepting the resistent disequilibrium states in the system, may account for the divergence of empirical results<sup>118</sup>. Also operational definitions are a source of disagreement. For instance, the empirical definition of money has presented problems in connection with the validation of alternative theories of the transmission mechanism. The complementarity or substitutability of money - an important factor in delineating the scope of the test - depends on the definition money is given. Finally, the selection of evidence is determined by the degree of simplicity or aggregation of the models used.

In evaluating Krupp's explanation of controversy, there arise certain questions that somehow weaken the explanatory value of the factors presented. Contrary to what Krupp claims, for instance,

i.e. that Samuelson is a Logical Positivist and an ultra-empiricist, whereas Friedman is not (!)<sup>119</sup>, I think that it can be discerned from the methodological writings of either economist that they both belong to Positive economics<sup>120</sup>. If this is so, then both believe in the ultimate power of empirical testing. Moreover, although they accept the existence of value judgements, they cannot accept, since they are Positive economists, that because of them, controversial issues cannot be resolved with empirical facts. They believe that empirical testing will potentially resolve the controversy. And this is true despite the fact that the Monetary controversy persists and survives all the empirical tests used by either faction. The result is that the validation procedure in the controversy is hampered not only by the intermediation of values and preconceptions, but also by the application of an epistemological datum, i.e. empirical testing, which, in itself, may be methodologically unjustifiable and, even more, has been shown inadequate as a criterion of theory validation in the case of the Monetary controversy. While, therefore, Krupp explains the divergence of opinions in economics and the possibility of methodological deadlock, in terms of obstacles in scientific procedure, he does not explain the stubborn persistence of economists involved in the controversy in continuing to use the same criterion of validation and objectivity. Consequently, although Krupp's explanatory schema may help to account for the difficulty of resolving the controversy with the existing methodological means, it cannot fully account for the impossibility of resolving it. Value judgements may explain differences in opinion but they do not fully explain their persistence. In this case a different explanation is required, one

that would account for both the divergence of opinions and also the mechanisms that perpetuate such divergence.

Another difficulty in understanding Krupp's explanation is that although in his framework values, preconceptions and judgements permeate all the stages of the validation process he, nonetheless, ascribes to controversy the property of being a vehicle for scientific advance and issue clarification<sup>121</sup>. In fact, because of this property, controversies are considered essential for scientific procedure<sup>122</sup>. Krupp justifies this position by evoking the role of controversy as an agent of advance in scientific knowledge<sup>123</sup>. However, the difficulty is that if, according to Krupp, deductive criteria, i.e. strategy, operational definitions, conditional statements, are disputed because they are value-laden, and if inductive criteria, i.e. selection of empirical evidence, are also disputed because of "delicate judgements", then one cannot see what criteria are left for determining what is progress in scientific knowledge and what is not. Moreover, the hypothesis 'controversy-scientific advance' is not always supported by the facts. Not all controversies in economics have brought scientific advance. For example<sup>124</sup> the fact that the Marginalists won over the controversy with the Historicists, and became the established paradigm, does not mean that issues have been clarified and that economic knowledge approximated truth more scientifically. Indeed, problems and phenomena are looked at differently but not necessarily in a 'better' or 'clearer' way. Following Kuhn's analysis, a paradigmatic shift involves a complex process, in which controversy, as an 'anomaly', plays a certain role, but also in which a host of other factors play an

important role<sup>125</sup>. Moreover a paradigmatic shift, according to Kuhn, does not necessarily imply scientific advance, but only a change in perspectives and world views<sup>126</sup>. Furthermore, socio-economic forces, rather than only controversy, can account for the paradigmatic shift and the development of economics, for instance the rise of Marginalism reflecting the rise of industrial capitalism. In the case where Marginalism won over Historicism, there was not necessarily scientific progress, but perhaps only the suppression of one opposing school of thought (unfavoured by the dominant class) to the advantage of another (apologetic to the dominant class)<sup>127</sup>.

In concluding, I maintain that we cannot distinguish between scientific progress and scientific regress when criteria are blurred by ideological struggle. Scientific advance for whom? There is no automatic mechanism, as Krupp implies<sup>128</sup>, that will turn controversies into scientific progress. Paradoxically, Krupp himself admits that criteria of theory selection are a source of conflict<sup>129</sup>. It is difficult, therefore, to understand what criteria he uses in order to determine scientific advance. Finally, although Krupp explains the origin of controversy and identifies the sources of disagreement in economics, he does not, however, fully explain persistent controversies and the possibility of continuing theoretical and methodological stalemate.

## FOOTNOTES

## PART III

## OUTLINE

1. "For the most part, the crucial question [is], 'What observed facts would contradict the generalization suggested, and what operations could be followed to observe such critical facts?'"', M. Friedman, "Lange on Price Flexibility and Employment: A Methodological Criticism", in his Essays in Positive Economics, 1953, p.283. And, "Every science is based squarely on induction, on observation of empirical facts . . . All sciences have the common task of describing and summarizing empirical reality.", P.A. Samuelson, "Economic Theory and Mathematics: An Appraisal", American Economic Review, 1952, p.57.
2. "Keynesians and Monetarists generally agree as to the nature of their disagreement and the kinds of tests that are likely to help resolve the controversy", Ward, "What is Wrong with Economics", op.cit., p.11.
3. P.A. Samuelson, "Reflections on the Merits and Demerits of Monetarism", in Diamond, "Issues in Fiscal and Monetary Policy . . .", op.cit., p.9.
4. Friedman, "Monetarism Vs Fiscalism", op.cit., p.58.
5. According to Culbertson, "The existing research methodology lacks defenses against bias. Since good fits and statistically significant results can . . . be found for models leading to different estimates of parameter values", "Macroeconomic Theory . . .", op.cit., p.107.

## CHAPTER 6:

1. F. Machlup, "Positive and Normative Economics, An Analysis of the Ideas", in Heilbroner's, Economic Means and Social Ends, op.cit., pp.99-129.
2. G. Myrdal, The Political Element in the Development of Economic Theory, 1953, p.19 and pp.23-29.
3. G. Myrdal, "How Scientific are the Social Sciences", Economie et Sociétés, Aug.1972, pp.1478-9.
4. Myrdal, "The Political Element . . .", op.cit., p.XIII.
5. M. Friedman, "The Methodology of Positive Economics", op.cit., pp.4-5.
6. Ibid., p.4. Also M. Friedman, "Value Judgements in Economics", in S. Hook (Ed), Human Values and Economic Policy, 1967, p.85.

## FOOTNOTES (cont.)

## PART III: CHAPTER 6:

7. Friedman, "Value Judgements . . .", op.cit., p.87.
8. See Myrdal, "The Political Element . . .", op.cit., p.11.
9. An exception is Machlup who does study the various meanings of value judgements, see for example, "Positive and Normative Economics", op.cit..
10. Friedman, "The Methodology . . .", op.cit., p.5.
11. See Myrdal, "The Political Element . . .", op.cit., p.192.  
Also P. Streeten, Values in Social Theory, Introduction, 1958, p.XXI.
12. Myrdal, ibid., p.X.
13. Ibid., p.36.
14. See Streeten, ibid., p.XIV, p.XV, p.XXIV and p.XXVIII. Also Myrdal, "Ends and Means in Political Economy", in Streeten, ibid., p.206 and p.211.
15. G. Myrdal, "The Logical Crux of All Science", in Streeten, ibid., p.233.
16. Streeten, ibid., p.XIII and p.XI and p.XVIII.
17. Moreover, according to Streeten, the act of making a classification or a logical distinction, such as ends and means or positive and normative statements, does not render economic theory ethically neutral (see ibid., p.X]III).
18. J. Robinson, Economic Philosophy, 1962, K. Boulding, "Economics as a Moral Science", American Economic Review, 1969, "The Basis of Value Judgements in Economics", in Hook's, Human Values and Economic Policy, op.cit., R. Meek, "Economics and Ideology", in his Economics and Ideology and other Essays, 1967, "Value-Judgements in Economics", British Journal for the Philosophy of Science, 1964, M. Rothbard, "Value Implications in Economics", American Economist, 1973.
19. Robinson, ibid., p.7.
20. Ibid., p.8.
21. ibid., p.117 and p.124.
22. ibid., p.19.
23. Ibid., p.28.

## FOOTNOTES (cont.)

## PART III: CHAPTER 6:

24. Ibid., pp.18-9.
25. Ibid., pp.126-7.
26. Ibid., p.8.
27. Ibid., p.8.
28. Ibid., p.9.
29. Ibid., p.9.
30. Ibid., p.9.
31. Ibid., p.28.
32. Ibid., p.19.
33. Myrdal, "The Political Element . . .", op.cit., p.18, also p.VIII and pp.192-3.
34. Ibid., p.192.
35. Myrdal, "How Scientific . . .", op.cit., pp.1486-7.
36. "International Integration", in Streeten's, "Values in Social Theory", op.cit., p.1.
37. "The Logical Crux of All Science", op.cit., p.233.
38. "How Scientific . . .", op.cit., p.1490.
39. "Economics-Science, Ideology, Philosophy?", Scottish Journal of Political Economy, 1963, p.217.
40. "Economics and Ideology", op.cit., pp.223-4. See also Bouding, who says that, "science has an essential ethical basis . . . Under these circumstances science cannot proceed at all without at least an implicit ethic . . .", "Economics as a Moral Science", op.cit., pp.2-3. Also Streeten says that, "Awareness of his own valuations, and of the limitations of his conclusions, are among the theorist's safeguards against falling into the ideological trap", "Introduction", op.cit., p.X]VI. Also Hill says that, "value judgments must be made explicit . . . Both intellectual honesty and professional responsibility demand explicit recognition of the normative preconceptions of every theory", "A Critique of Positive Economics", American Journal of Economics and Sociology, 1968, p.265.
41. "Value Implications of Economic Theory", op.cit., p.35.

## FOOTNOTES (cont.)

## PART III: CHAPTER 6:

42. I.N.D. Little, A Critique of Welfare Economics, 1950, p.79 and p.81, footnote 1.
43. Robinson, ibid., p.27.
44. Ibid., p.8.
45. Ibid., pp.8-9.
46. Ibid., p.28.
47. See, ibid., p.26, and her Introduction to Modern Economics, 1974, pp.2-3.
48. Myrdal, "How Scientific . . .", op.cit., p.1482, and p.1484 and p.1492. See also his "International Integration", op.cit., p.4. Also Meek formulates a similar criterion in his, "Value Judgements in Economics", op.cit., p.92.
49. Streeten, ibid., p.XI.
50. "the criterion of its truth can never be anything other than the pragmatic one of its usefulness in bringing our observations of facts into a meaningful and non-contradictory system of knowledge", Myrdal, "The Logical Crux . . .", op.cit., p.233.
51. An even clearer example is the difference between Neoclassical economics and Marxist theory. In the latter case, research on surplus value or of the degree of domestic and international exploitation is considered relevant, whereas for orthodox economics equilibrium theory prevents the asking of such questions and does not even accept and use such terms.
52. See Hollis and Nell, "Rational Economic Man", where they ask the question of "what is relevance?", p.39.
53. "The evidence is systematically selective", Myrdal quoted in W.A. Leeman, "The Status of Facts in Economic Thought", op.cit., p.409.
54. Myrdal, "The Logical Crux of All Science", op.cit., p.233.
55. Streeten, op.cit., p.XII.
56. See Macfie, op.cit.. Also Linstromberg comments that, "Relativists urge . . . an approach that introduces values explicitly into the analysis", "The Philosophy of Sciences and Alternative Approaches to Economic Thought", Journal of Economic Issues, 1963, p.178 also p.190. See also Streeten who says that, "Once the dichotomy between values and facts is rejected, all kinds of questions about Myrdal's own method arise . . . To admit any premises as equally valid would be to fall victim to relativist liberalism", Introduction in "Value in Social Theory", op.cit.p.XIV.

## FOOTNOTES (cont.)

## PART III: CHAPTER 6:

57. "The Political Element . . .", op.cit., p.206.
58. Ibid., p.206.
59. Streeten, op.cit., p.X]. Or as Knight says: "Scientific activity is the pursuit of truth. And truth is essentially a value, not merely a want; and still less it is merely an effect in a causal sequence", F.H. Knight, On the History and Method of Economics, 1956, p.132.
60. Also Streeten speaking of Myrdal says that, "Unlike many methodologies who are quite naive about their own theory, Myrdal gradually develops a kind of a methodology of methodology - a critical theory that criticises itself", (Streeten, op.cit., p.XII).
61. "The Methodology of Positive Economics", op.cit., p.25. Or he claims that "more than other scientists, social scientists need to be self-conscious about their methodology", ibid., p.40.
62. "The Political Element . . .", op.cit., p.22.
63. Culbertson, "Macroeconomic Theory . . .", op.cit., p.9 and p.537.
64. An interesting hypothesis about controversy and crisis in economics is put forward by Myrdal, who argues that, "when economists within (the establishment) disagree, which they often do, there is usually enough of a common basis of thought not to create serious disturbance within the camp. The disagreements help protect them from seeing that together they form the establishment", in, Against the Stream: Critical Essays on Economics, 1972, p.2.
65. See, e.g., A.W. Coats, "Is There a Structure of Scientific Revolutions in Economics?", KYKLOS, 1969, pp.289-294.
66. Nonetheless there is the example of C.E. Ferguson who admitted that he acts on faith in neoclassical theory, The Neoclassical Theory of Production and Distribution, 1969, p.XVII.
67. See, e.g., J. Kornai, "Anti-Equilibrium", 1971, p.4, p.7 and pp.8-9. N. Kaldor, "The Irrelevance of Equilibrium", Economic Journal, 1972, pp.1237-1252, P. Sweezy, "Modern Capitalism", 1972, pp.54-5, p.59, and p.63, Heilbrunner, "Is Economics Relevant?", op.cit., p.XI and p.XV, Hill, "Towards A Critique of Positive Economics", op.cit., pp.263-5. For a discussion of the equilibrium concept see A. Coddington, "The Rationale of General Equilibrium Theory", Economic Inquiry, 1975.
68. Friedman, "The Methodology . . .", op.cit., pp.20-1. We have seen, however, the ambiguity involved with the truth value of "as if" assumptions.

## FOOTNOTES (cont.)

## PART III: CHAPTER 6:

69. See, e.g., Heilbroner, "On the Limited Relevance of Economics", The Public Interest, 1970, pp.80-93, F. Machlup, "Are the Social Sciences Really Inferior?", in M. Natanson's, Philosophy of the Social Sciences, 1963, pp.158-180, Brown, "The Underdevelopment of Economics", op.cit., K. Boulding, "The Verifiability of Economic Images", in Krupp's, "The Structure of Economic Science", op.cit., pp.129-144, Lowe, "Toward A Science of Political Economy", op.cit., pp.5-6 and p.10.
70. F.A. Hayek, "Scientism and the Study of Society", Economica, 1942, 1943, 1944-45, pp.267-291, pp.34-63, pp.27-39 respectively.
71. F.H. Knight, On the History and Method of Economics, 1956, pp.121-227.
72. "Economics and other social sciences deal with knowledge and truth of a different category from that of the natural sciences, truth which is related to sense observation - and ultimately even to logic - in a different way from that arrived at by the methodology of natural sciences", Knight, ibid., pp.154-5, see also Hayek, 1943, pp.40-2 and 1942, pp.279-80, and L. Nabers, "The Positive and Genetic Approaches", in Krupp's, "The Structure of Economic Science", op.cit., p.72.
73. Heilbroner, "On the Limited 'Relevance' of Economics", op.cit., p.89, Coddington, "Positive Economics", op.cit., p.9.
74. See E. Grunberg, "Notes On the Verifiability of Economic Laws", op.cit., pp.337-341.
75. See Fayerabend, "Problems of Empiricism", in Colodny's, "Beyond the Edge of Certainty", op.cit., pp.174-5, and Linstromberg, "The Philosophy of Science and Alternative Approaches to Economic Thought", op.cit., pp.186-9.
76. Knight, ibid., p.163.
77. K. Boulding, "Is Economics Culture Bound?", American Economic Review, 1970, Papers and Proceedings, pp.406-11, also his "Beyond Economics: Essays on Society, Religion and Ethics", 1968, pp.110-111. Also Heilbroner, "Is Economic Theory Possible", Social Research, 1966, pp.272-294.
78. See Coddington, "The Rationale of General Equilibrium Theory", op.cit., p.3, where he speaks of the "reproductive fallacy". See also Machlup, "Theories of the Firm . . .", op.cit.
79. Sweezy, "Modern Capitalism", op.cit., p.57, Grunberg, ibid., pp.337-348. For a fuller discussion of open systems and experimentation see Grunberg, ibid., Bhaskar, "A Realist Theory of Science", op.cit., pp.12-24 and Winch, "The Idea of Social Science", op.cit., pp.66-91 and pp.121-136.

## FOOTNOTES (cont.)

## PART III: CHAPTER 6:

80. Hayek, 1942, p.277 and 1943, p.283.
81. Knight, ibid., p.239.
82. Heilbroner, "On the Limited Relevance . . .", op.cit., p.90, and, "Is Economics Relevant", op.cit., p.XIX.
83. Knight, ibid., pp.175-7 and pp.121-4. However, as Knight himself qualifies in p.160, this argument is not absolutely true where Heisenberg's Principle of Uncertainty applies, in which case the action of observation and experiment interferes with the phenomenon, thus changing it unpredictably. (See P. Feynman, The Character of a Physical Law, 1965, pp.143-4 and A.F. Chalk, "Concepts of Change and the Role of Predictability in Economics", History of Political Economy, 1970, pp.97-116.
84. ". . . the difference between predicting human behaviour and predicting the behaviour of physical objects under changed conditions [is] in that the latter neither behave irrationally or sentimentally, nor make mistakes, nor 'change their minds' . . . as human beings are notoriously liable to do so", Knight, ibid., p.175, also pp.126-7.
85. Knight, ibid., pp.123-4.
86. Ibid., p.156 and p.159.
87. Hayek, 1943, p.37.
88. Hayek, 1943, p.37.
89. See Coddington, "Positive Economics", op.cit., p.13. In connection with this Coddington offers an explanation of the irresolution of controversies in economics in terms of the inability of Positive economics to define what counts as evidence (p.9). According to Coddington, the consequential activity of testing with empirical evidence may fail because of defects such as that, "the concepts of the theory may be vague; the data may be unreliable, or they may have a spurious precision unwarranted by the clarity of the underlying concepts", (p.11). Given however a potential (and hypothetical) sharpening of the concepts and definitions used in Positive economics, the question is: would controversies persist? I believe that they would since (a) controversies in Positive economics stem from its structure, and thus form necessary parts of it, and (b) a further sharpening of definitions would meet ambiguities whose elimination would cause the structure of Positive economics to collapse. The explanation put forth in the next chapter discusses these possibilities.

## FOOTNOTES (cont.)

## PART III: CHAPTER 6:

90. Friedman, "The Methodology . . .", op.cit., p.10.
91. A. Marshal, Economics of Industry, 1907, pp.19-22.
92. Hayek, 1943, pp.42-3.
93. See Hollis and Nell, "Rational Economic Man", op.cit., p.13.
94. Ibid., pp.95-113.
95. Hayek, 1943, p.43. There is fallacy, however, with respect to the differentiation between individual and whole. Firstly, the use of the term individual implies that there is something which is not individual, i.e. something that is the opposite of individual, namely wholes. And secondly, the individual can be interpreted as an aggregate unit, that is to say, as a whole containing a collection of attributes, which in themselves may be wholes.
96. Ibid., 1942, pp.281-3.
97. Ibid., 1942, pp.257-277.
98. Ibid., 1942, p.277.
99. Ibid., 1942, p.283.
100. Knight, "On the History and Method . . .", op.cit., p.124.
101. Ibid., p.156 and pp.157-9. "In the field of human interests and relationships much of our most important knowledge is intensely non-quantitative and could not conceivably be put in quantitative form without being destroyed . . . in the absence of any technique of measurement, there is no clear differentiation between a subjective and an objective quality, and the reference of an experience to the external world or to the mind is shifting and largely arbitrary", ibid., in footnote 10, p.166 and in p.167.
102. Ibid., p.169.
103. Ibid., p.156.
104. Winch, "The Idea of Social Science", op.cit., pp.32-3.
105. Knight, ibid., pp.156-7.
106. Ibid., p.133.

## FOOTNOTES (cont.)

## PART III: CHAPTER 6:

- 107. Knight, ibid., p.156.
- 108. Ibid., p.133 (in footnote 5).
- 109. S. Krupp, "Types of Controversy in Economics", in his, "The Structure of Economic Science", op.cit., pp.39-51.
- 110. Ibid., pp.40-1.
- 111. Ibid., p.41.
- 112. Ibid., p.41.
- 113. Ibid., p.44.
- 114. Ibid., p.45.
- 115. Ibid., p.45. Also Leeman shares the same criticism when he argues that, "There will be unresolvable and divergent schools of thought in economics as long as assumptions are discussed in a vacuum, where appeal to fact is prevented by laws of tendency and by *ceteris paribus* clauses", *The Status of Facts in Economics*, op.cit., p.411.
- 116. Krupp, ibid., p.46.
- 117. Ibid., p.50 and p.51.
- 118. "People who do accept the efficacy of competitive equilibrium pricing as an effective system of explanation and those who do not differ greatly in their treatment of empirical matters . . .", (Krupp, ibid., p.50).
- 119. See p.43, esp. footnote 4. For Krupp a Logical Positivist is one who wants to test assumptions empirically, whereas somebody, like Friedman who wants to test predictions empirically, is not.
- 120. Friedman: "The Methodology of Positive Economics", op.cit., "Value Judgements in Economics", op.cit., "Lange on Price Flexibility and Employment", op.cit.. Samuelson: "Foundations of Economic Analysis", op.cit., Introduction and chapter II, "Discussion", American Economic Review, Papers and Proceedings, 1963, "Economic Theory and Mathematics: An Appraisal", American Economic Review, 1952. Ferguson, for instance, says that the "methodological position . . . labelled 'logical positivism' . . . finds wide acceptance among modern economists. For example P.A. Samuelson . . . M. Friedman", "Microeconomics", op.cit., p.6.

## FOOTNOTES (cont.)

## PART III: CHAPTER 6:

121. Krupp, ibid., pp.39-40.
122. Ibid., p.40 and p.51.
123. Ibid., p.42.
124. The same example is used by Krupp, (p.42).
125. Kuhn, "The Structure of Scientific Revolutions", op.cit., p.91.
126. Ibid., p.94 and pp.198-204.
127. See Hunt and Schwartz, "A Critique of Economic Theory", op.cit., pp.12-15. Although Marginalism is the dominant school of thought, Historicism in the form of Marxism and Institutionalism, still survives and persistently questions the scientific merit of Neoclassical economics.
128. Krupp, ibid., pp.30-40.
129. Ibid., p.40.

CHAPTER 7:

THE LOGIC OF CONTROVERSY

IN POSITIVE ECONOMICS

"A contradiction originally latent, gave rise to opposed schools of thought."<sup>1</sup>

G. Myrdal

"There is a conflict between contrary tendencies, each of which is necessary to existence, and there must be a set of rules to reconcile them."

"The leading characteristic of the ideology that dominates . . . is its extreme confusion. To understand it means only to reveal its contradictions."<sup>2</sup>

J. Robinson

## CHAPTER 7:

THE LOGIC OF CONTROVERSY IN  
POSITIVE ECONOMICS

## A. CHOICE OF METAPHYSIC

"Car les méthodes impliquent des métaphysiques, elles trahissent à leur insu les conclusions qu'elles prétendent parfois ne pas encore connaître . . . La méthode définie ici confesse le sentiment que toute vraie connaissance est impossible."<sup>3</sup>

A. Camus

Like any other myth Positive economics - in both its epistemological and ontological aspects - expresses a wish. This wish is to know and control reality. Like all myths however, Positive economics is ambiguous. The function of this ambiguity is to conceal the contradiction that arises from the ideals of Positive economics and its practice. The ideals are that theory and fact, subject and object, ends and means, are truly independent and, more importantly, that empirical testing ought to discriminate between competing theories. The practice is that theory and fact are interrelated in an ambiguous way, that the subject is simultaneously the object, that ends influence means and finally, that in actual fact controversies persist in Positive economics in spite of empirical testing.

The purpose of this chapter is to show the logic that binds the structure of Positive economics and is behind the ambiguities concealing this contradiction. In doing this an explanation of perpetual conflict in economics will emerge as well as of the paradox between the continuous faith of Positive economists to the positivist ideals and the reality of persistent theoretical strife.

One point that must be emphasized from the outset is that this explanation does not intend to offer an account of the factors that cause the emergence and persistence of controversies. Instead it attempts to seek a ubiquitous relation that connects the distinctions of Positive economics. This relation will be defined according to each particular distinction as this is stipulated in Positive economics. No other factor will be added but solely the logic of the categories of Positive economics will be interpreted. The totality of these relations in the whole of Positive economics will form a logical structure that can be viewed as a deep structure lying behind the categories, distinctions and thought patterns of Positive economics. It will be hypothesized that conflict in Positive economics is a manifestation of this deep structure. Thus an analysis of the surface structure of Positive economics will reveal a deeper structure, which through logical transformations and correlations will be shown to manifest itself in irreconcilable conflict.

The validity of this method, and thus of this explanation, is not based upon an external criterion, such as empirical facts or relevance, but on its esoteric cohesion and plausibility. Plausibility, however, is only a contingent characteristic of this explanation. Thus it is not required necessarily to render it valid. The validity of the explanation depends on its logical coherence in revealing the deep structure of Positive economics. And even at that it does not claim to be more real than any other explanation, but only that it can be used as a structural perspective through which to look at continuous conflict. Needless to say that as Positive economics cannot justify

its method of empirical testing, due to the Problem of Induction, so the method used here cannot justify why one account of the logic of conflict equally logically coherent is more valid than another, due to the Problem of Deduction. The only (and perhaps arbitrary) reason for choosing this particular method is that it offers a viable (a priori) means to structure the pieces of conflict in economics logically.

Thus, the logical justification of any methodology involves contradictions due to unresolved problems in the theory of knowledge. Difficulties and ambiguities in the logical structure of methodologies reflect difficulties and ambiguities in the justification of induction and deduction. The consequence of these difficulties is to make, as Streeten says, "an arbitrary step into metaphysics"<sup>4</sup>. Perhaps the only way out of such difficulties is to acknowledge them. My task, however, is not only to uncover the metaphysic and use it as a 'world-view', as Robinson and Myrdal maintain, but also to accept the fact that if I want to put forward an explanation of conflict in economics, I must do so within the logical constraints of my metaphysic. In general my metaphysic is that since there are logically unresolved ambiguities in the theory of knowledge, no methodology (including mine) can be logically justified in its claims to knowledge (irrespective of whether it applies correctly, as in the case of induction which although in general it has been shown to work well, at least as far as physics is concerned, it has not been possible to justify logically). However, since economics (and science in general) has to have claims to knowledge, it has to act despite epistemological difficulties. The same situation applies to my explanation only with the difference that

my metaphysic accepts the contradiction between logical impasse and necessary action as part of its structure.

In what follows I offer an explanatory schema of conflict in economics that although it is viable cannot offer (in a logical sense), due to the above-mentioned problems, any validational status other than its explanatory and structural consistency. Furthermore this viability is enhanced by the fact that there are no alternative structural explanations of conflict in Positive economics. Most explanations attribute conflict to historical, sociological or ideological reasons. In this sense an external view of Positive economics is taken. I consider the explanation proposed here as alternative to these explanations, since it attempts to look at conflict in Positive economics from an internal point of view. Thus, until a different internal and structural explanation is proposed it may serve as a provisional one, which however cannot be logically justified and validated, unless an arbitrary and logically contradictory step is taken. It follows that I cannot use the arguments that it explains 'better' or 'more adequately', or that it is 'more relevant', simply because I cannot have unequivocal criteria of 'betterness', 'adequacy' and 'relevance'. The choice of the particular method used in this explanation is not therefore dictated by any logically justifiable methodology, but by a metaphysic (itself contradictory) which stipulates an approach incorporating an emphasis on aesthetic structure (in the structuralist sense), a symmetric consistency of explanation and a logical viability. If one argued, however, that the criteria of

aesthetic, symmetrical and viable are subjective, the answer would be that they are stipulated within the postulated metaphysic (in an axiomatic sense), which can be contested only by an appeal to an alternative, subjective and, given the Problems of Induction and Deduction, unjustifiable metaphysic. If one argued back saying that this might lead to relativism, then the answer would be that it does so out of the logical necessity of having to act (i.e. give an explanation) and the contradiction that one cannot logically justify why one acts, and whether the particular action (explanation) is valid. Perhaps the only validity and justification that can be claimed for the following explanation is the absence of other similar explanations and its viability to accommodate persistent theoretical and empirical conflict.

#### B. AN ONTO-LOGICAL EXPLANATION OF THE MONETARY AND F-TWIST CONTROVERSIES<sup>5</sup>

In what follows I shall attempt to put the distinctions of Positive economics in relation to one another, according to a unifying principle. This principle will take the form of a structural opposition between necessity and contingency that characterizes all the distinctions. In appearing in various combinations and forms throughout the structure of Positive economics, it will constitute a deep structure beneath the distinctions that can be used as a basis for understanding the logic of conflict in Positive economics.

By being a fusion of positivist philosophy and Neoclassical economics, Positive economics inherits a fundamental dichotomy, first,

at the methodological level, and second, at the ontological level. At the methodological level, like any other philosophical system, Positivism is confronted with the dilemma between universal and particular knowledge. Essentially, it has to find a way out of the dilemma by choosing between either a totally abstract and general solution or a totally empirical and specific one, that is between either an absolutely deductive or an absolutely inductive perspective.

This problem has confronted most philosophical systems and up to the 16th and 17th centuries its solution followed, primarily, the Aristotelian lines of 'essence' and 'appearance'. The solution to the dilemma, according to the ancient and medieval philosophies, was to go beyond sense observations (appearances) because they were temporal and contingent, in order to unravel an omnipresent and everlasting 'essence' the existence of which was considered necessary. The proof of the validity of a statement about the existence of 'essence' depended upon logical and deductive arguments<sup>6</sup>. However, the infinite regress, created from having to justify the choice of one particular 'essence' among others, could only be stopped by an appeal to a (superimposed) metaphysic, or some 'imperative', that would bring the regress into a 'terminal essence'.

Positivism (in all its forms) emerged as a solution to the dilemma mainly as a reaction against such a type of metaphysical reasoning<sup>7</sup>. As science developed the need for observation and experiment increased<sup>8</sup>. It was believed that theories should not have metaphysical foundations but that their truth should correspond to

empirical reality<sup>9</sup>. In a sense, this reaction led Positivists to the other horn of the dilemma, namely the Problem of Induction. In other words, how could one know that a set of empirical evidence was not specific to a particular context and therefore might change from situation to situation. Or, put differently, one needed to prove the general validity of empirical evidence by an appeal to another set of empirical evidence which would prove the general applicability of the initial set.

However, Positivists confronted these difficulties by attempting to make a clear distinction between the two extremes of deductivism and inductivism. They made a breakthrough (or so they thought) by introducing a fundamental dichotomy that runs through the structure of positivist epistemology. They postulated a theory of knowledge that is distinct from a theory of reality. By sharply distinguishing what is to be reality and what knowledge of it, a distinction of validating roles was made possible. The role of knowledge (theory) is strictly to sort out the complex reality, while the role of validation is given over to empirical experience derived from that reality. Thus as only empirical reality retains the role of validation, experimentation and observation become the indispensable tools of science and 'true' knowledge. There are no mystical connections between theory and fact. The role of each one is clearly postulated and distinguished. In doing this Positivism formed the basis for the erosion of all metaphysical and transcendental tendencies inherited from older philosophies. There was to be no fixed idea any more - such as God or 'essence' - that would render

a theory valid. There was only scientific observation and experiment that decided any theoretical issue. In the positivist world the distinction between 'essence' and 'appearance' became obsolete<sup>10</sup>. Although Positivists would attempt to impute causation to unobserved phenomena, they would reject any mystical or occult entities - such as 'essence' and 'appearance' - that could not be put to any testable and observable form<sup>11</sup>. In their attempt, therefore, to overcome epistemological difficulties stemming from the Problem of Deduction and to divest their assumptions of any metaphysical remnants, Positivists constructed a philosophical system that offers a set of prescriptive rules for the acquisition of scientific knowledge (empirical testing) and a set of views that define the nature of the world (complex, unmechanistic, reality)<sup>12</sup>.

As an outcome of such sharp distinctions between knowledge and reality, of theory and fact, Positivism inherited an opposition of tendencies that formed the basis of its epistemological premises. On the one hand, its claims to scientific knowledge consist of principles the application of which is considered necessary for the acquisition of 'true' knowledge. On the other hand, reality is seen as a collection of contingent events the occurrence of which obeys no necessary and mechanistic rules.

Concerning epistemological principles, Positivists stipulate a set of rules that are recognized as universal. No matter what the nature of the phenomena to be studied is, be it human or physical, the method of studying them is common to all. The application of this

method does not depend on the historical context or particular nature of the events under study, but rather on a strict observance of the prespecified itinerary: observe - make hypotheses - test. In this sense, therefore, Positivism has as its subject a set of rules the application of which leads necessarily to the truth or falsity of a theory (see Figure VI on p. 308).

Concerning reality, the object of Positivism, it consists of unrelated events the nature of which is dependent upon temporal and provisional contexts. The world is made of a set of contingent states which are not causally related in any necessary way. Occurrences in this complex world are not considered to follow mechanistic rules or to rely on a certain, prespecified structure. In view of such a chaotic nature of reality, the task of the scientist becomes to explore systematic patterns and regularities through observation and experiment. However, one is not supposed (allowed) to attach mechanistic causality to these patterns as this would go against the contingency of reality. Patterns could be systematic and be used as projectors of future events, but they do not reflect any causal functioning. Causal relations are to be found only in the hypothetico-deductive nature of theories. Reality is far too complex and accidental to allow actual inferences of causal relationships between real events. Causality is reserved for general theories, and they are mostly heuristic. In short, the central theme of the positivist theory of reality is that any real event in the world is contingent and could be otherwise than what it is<sup>13</sup>.

From one point of view, in having made a clear distinction between knowledge and reality or theory and fact, Positivism succeeds in reconciling the (empirically) untested certainty of absolute deductiveness and the specific uncertainty of absolute inductiveness. By postulating a fixed and independent set of rules as a guide to know the world it renders the study of a contingent world possible. For other systems, for example, knowledge and reality, theory and fact, are intimately interrelated and the first constitutes part of the latter. Marxism, for instance, explains its epistemological position, or the position of others, in terms of the historical context in which both theory and fact are moulded. In other words each historical period emits, as it were, its own theories and facts. The subject acts upon these facts in order to understand them as well as to change them. Knowledge and reality are one in the concept of 'praxis'. Whereas, in the case of Positivism it becomes essential for statements about knowledge to be different from statements about reality. In this sense, the opposition between necessary knowledge and contingent reality is needed to sustain the viability of the positivist epistemological position. From another point of view, Positivism by virtue of this juxtaposition between knowledge and reality, retains a fundamental tension which is to be found at all its levels. The recurring theme of necessity and contingency implied in the knowledge-reality dichotomy is further repeated in the analytic-synthetic and in the normative-positive distinctions forming a binding relationship. In what follows I shall describe how this tension is manifested in the structure of Positivism and relates to Neoclassical economics to form Positive economics.

## C. THE STRUCTURAL RELATIONS OF POSITIVE ECONOMICS

To begin with there is the distinction between knowledge and reality. Reality is there and must be known. However, as we have seen in chapter 2, the attempt to know reality gives rise to problems and logical impasses, i.e. the Problems of Induction and Deduction. To mediate for these impasses Positivism achieves a split between what is to be knowledge and what is to be reality. This split is achieved at the cost of an ambiguity in the definitions of knowledge and reality. In the case of knowledge, theory is given a definition which is a fusion between the abstract and the empirical. In the case of reality, facts are given a definition which is a fusion between the empirical and the abstract. The consequence of this ambiguity is a tension that needs to be relieved if the practice of Positive economics is to remain viable. This relief is realised in the joining of forces between Positivism and Utilitarianism. The ambiguity, however, is not dissolved entirely, but in this unification it appears as a structured dichotomy between necessity and contingency, which is manifested at, and permeates, all the levels of Positive economics. But let us see first how the unification between Positivism and Utilitarianism is realised.

In its application to the social sciences, Positivism finds its counterpart in utilitarian philosophy and Liberalism, of which Neoclassical economics is a byproduct<sup>14</sup>. In Utilitarianism society is split into atoms or individuals whose goal is the attainment of the greatest possible state of happiness or pleasure. In striving to attain this end individuals, free from any socially superimposed constraint, achieve the maximum of social happiness. The assumption

behind this belief is that since society is defined as the sum total of all individuals, the achievement of the greatest pleasure of each unit will automatically lead to the greatest pleasure of all units.

The synthesis between Positivism and Utilitarianism becomes feasible because the latter offers a theory of action (behaviour, reality) which suits the epistemology of the former<sup>15</sup>. Since individuals are considered discrete units and their action is gauged in terms of their observable behaviour to achieve a state of happiness, the study of society becomes amenable to scientific observation and experiment. The definition of society, since it is considered to be the addition of all individuals, is to be divested of any metaphysical and untestable notion of a 'whole'. The 'whole' can be translated into each individual and the action of each individual can be observed empirically. If, therefore, the behaviour of each individual can be scientifically assessed, then the behaviour of society, mutatis mutandis, can also be assessed. There is no qualitative difference between the totality and its parts<sup>16</sup>.

The intellectual unification between Positivism and Utilitarianism is further made possible by the fact that Positivism, having rejected all previous philosophies as metaphysical, turned to Utilitarianism to find the earthly rationality necessary for the application of its epistemology. As Hollis and Nell say,

Logical Positivism . . . drew on support outside philosophy, when it banished organic as well as moral notions from the moral sciences. Reason was again enthroned in a new return to timeless calculation and universal experiment.<sup>17</sup>

The relationship between Positivism and Utilitarianism is further consolidated, with reference to Positive economics, when neoclassical theory is brought into the picture. In parallel with Positivism, Neoclassical economics rejects the classical economic doctrines on the grounds that the latter use metaphysical (and unmeasurable) concepts such as value<sup>18</sup>. The major assumption in Neoclassical economics, in contrast to Classical political economy, relates to the motives of the economic agents. Their behaviour is determined by their striving to maximize. Consumers maximize their (measurable?) utilities and producers their quantifiable profit. Neoclassical theory, being intimately linked to Utilitarianism, applies the principles of this system to explain economic behaviour (reality). The utilitarian assumption of man's pursuit of satisfaction as the ultimate goal applies to Neoclassical economics as the satisfaction of economic wants and objectives. That is, each good has a definite amount of utility and the economic agent attempts to achieve such a combination of goods, in accordance with his/her income, that will maximize his/her total utility. Although the modern theory of consumption and demand does not deal directly with utility but with preferences, nonetheless it rests its foundations on Utilitarianism and Bentham's 'felicific calculus'. Preferences have replaced utilities because of their properties of measurability without altering, however, the basic neoclassical framework. According to Meek, ". . . the expulsion of utility from value theory has not meant the expulsion of the presuppositions which were brought in with the utility theory"<sup>19</sup>.

The neoclassical theory of economic behaviour can be separated into two parts: the one refers to a subjective and the other to an objective part. The central assumption of Neoclassical economics is that individuals set their own ends and goals and attempt to allocate their scarce means in such a way as to fulfil these goals. The first part of this assumption concerns the setting and perception of specific goals by the individual and the second part concerns the application of definite, fixed rules, determined by scarcity, to accomplish these goals.

I call the first part subjective because in it is assumed that each individual or group sets a particular set of goals as an objective which may be different from the rest of the individuals or groups. For example, a consumer sets as his/her particular goal the satisfaction of his/her wants, while the producer the pursuit of profit, or the government attempts to reach full employment and/or stable growth, etc. Marshall, for instance, says that,

individual judgements as to the desirability of different social aims, or as to matters of fact which lie beyond the scope of any individual's special studies, should be clearly distinguished from those which claim to have been reached by scientific method.<sup>20</sup>

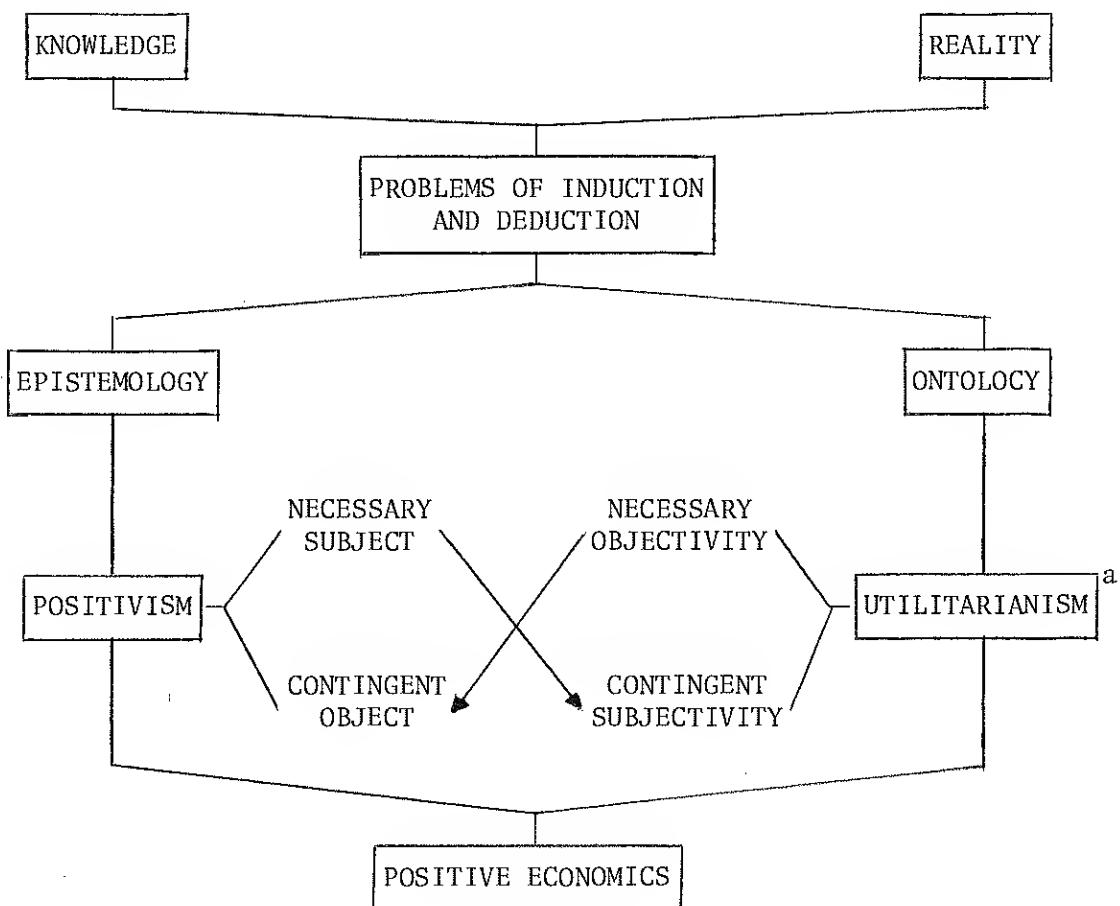
Thus these goals are set subjectively and are to be distinguished from objective (scientific) goals<sup>21</sup>. Also the content of each set of goals is different from individual to individual, from group to group, from sector to sector, from social role to social role, and from one historical period to another<sup>22</sup>. In this sense, therefore, the subjective determination of each set of goals is dependent upon

contingent factors. That is as periods, roles and sectors change so also does the content of the goals.

I call the second part objective because it transcends individual, sectoral, national or historical differences and applies universally. The second part shows how the social unit will act in a constraint situation<sup>23</sup>. It is a determined situation because of the assumptions of scarcity and economic rationality. The two assumptions together form the way the action of the economic agent will take; that is, he/she will always and everywhere allocate scarce resources in a rational way so as to achieve the maximum gains with the minimum of losses. In this sense the rational allocation of scarce resources to alternative ends constitutes a universal principle which is to be found outside the subjective scope of the individual. Its nature does not depend upon historical or contextual (contingent) factors and is therefore necessary. Constraint optimization is part of the necessary behaviour of any individual, sector, role and within any particular period and place<sup>24</sup>.

The result of the combination between Positivism and Utilitarianism (Neoclassical economics) is Positive economics<sup>25</sup>. In Positive economics one finds an epistemology based upon Positivism, and an ontology (a theory of social action) based upon utilitarian liberalism. The combination is realized at two (interrelated) levels: one logical and one contextual (historical).

FIGURE VI: NECESSITY AND CONTINGENCY IN POSITIVISM  
AND UTILITARIANISM



NOTES: a. In its manifestation in Neoclassical economics.

At the first level, the logical opposition between necessity and contingency in the positivist theory of knowledge is juxtaposed to the logical opposition between contingency and necessity in Utilitarianism (i.e. Neoclassical economics). The synthesis is feasible because of the complementarity between features in the positivist epistemology

and features in Utilitarianism. In Figure VI we see that the necessary subject of Positivism relates to the contingent subjectivity of Utilitarianism and the necessary objectivity of the latter relates to the contingent object of the former. This interconnection is realized in the overall context of a synthesis between an epistemology and an economic philosophy, or more broadly between a philosophy of scientific knowledge and a philosophy of reality. On the one hand, the economic philosophy offers the notion of a rational economic man applying his maximising rules in an objective (outside of him) manner and ordering the chaotic complexity implicit in the positivist contingent world. In this sense, the positivist rational scientist needs the utilitarian rational economic man to sort out his/her messy world. On the other hand, the positivist epistemology offers a theory of knowledge consisting of certain and fixed rules which put the conflicting multiplicity of possible alternative ends, forming the subjective part of rational economic man, into a theoretical perspective or a well-ordered framework<sup>26</sup>. Utilitarianism (or Neoclassical economics) views society in terms of sectors or individuals who share contingently determined values of varying importance and nature. In view of this it needs a theory of knowledge that would transcend ideological differences between individuals or groups and render the study of society scientifically viable. In this sense, rational economic man needs rational scientist man to sort out his 'irrational', subjective, part of the world<sup>27</sup>.

Accordingly, what is lacking from positivist philosophy is borrowed from economic philosophy and what is lacking from the latter

is borrowed from the former<sup>28</sup>. Positive economics is not, therefore, only a theory of knowledge or only an economic philosophy but is the unison of the two. It is a system embracing a positivist theory of knowledge and a utilitarian theory of economic reality, inheriting all the contradictions found therein. Although the tensions, found in Positivism and Utilitarianism separately, are relieved in the synthesis, they further reassert themselves in the formation of Positive economics. The logical opposition between necessity and contingency reappears and consolidates itself throughout the structure of Positive economics. Since the synthesis is achieved in terms of the juxtaposition between necessary objectivity (rules of behaviour, scarcity) and contingent object (reality) and between necessary subject (rules of science) and contingent subjectivity (alternative ends), the structured tension is filtered through and decomposed in the two foundations of Positive economics: science and behaviour. The viability of the system is therefore maintained with a renewed structured opposition inherited by the conjoining of parts of Positivism with parts of Utilitarianism. The connection of Positivism and Utilitarianism is in their philosophical alliance to combat the ambiguities inherited from the Problems of Induction and Deduction. In doing this, however, they do not resolve the riddle but rationalize it and reassert the ambiguities in a form of a structural relation, i.e. contingency-necessity. The connection is important because it implies the complementarity of the two systems. And the complementarity forms the context in which the structured contradiction or tension inherited reappears.

At the contextual (historical) level certain developments in both Positivism and Neoclassical economics facilitated the marriage of the two. Firstly, both Positivism and Neoclassical economics reject anything that has to do with metaphysics<sup>29</sup>. 'Essences' or 'wholes' are considered pseudoproblems<sup>30</sup>, and their study should be left to art rather than science<sup>31</sup>. In consequence, Positivism is led to develop a theory of knowledge based on methodological individualism and Neoclassical economics a theory of reality based on economic individualism. The compartmentalization of reality into discrete units renders scientific observation feasible. The actions of individuals are cast in terms of rational choices or preferences the classification of which enables the application of measurement techniques<sup>32</sup>. By divesting reality from notions such as 'whole' or 'essence' Positivism opens the way for experimentation and science<sup>33</sup>.

Another development that brought the alliance<sup>34</sup> closer together - also related to the 'anti-metaphysics' campaign - is the distinction between analytic and synthetic, on the one hand, and means and ends on the other. Although Positivists acknowledge the existence of metaphysical statements they render them harmless by ascribing to them a vacuous role. Such statements, it is believed, are empty because they do not correspond to any part of the factual domain. Some of them are also tautologous since their meaning depends on the parts from which they are constructed. The function of these statements should, therefore, remain heuristic and their analysis should be referred to the linguist rather than to the scientist. In contrast synthetic statements are about facts derived from the real

(empirical) world (we have seen, however, that each part of the distinction contains an ambiguity: analytic, THEORETICAL-empirical; synthetic, EMPIRICAL-theoretical).

This fundamental positivist distinction paves the way for the neoclassical distinction between ends and means. It helps the economist to distinguish which is to be the subject and scope of economics. It forms the boundaries within which economics would feel secure from whatever has to do with art and ideology. Economists are not to engage in questions concerning the preference of desirable ends; their job is rather to delineate the method by which to attain these ends<sup>35</sup>. Goals, aspirations or subjective ends are neither here nor there in terms of science. They belong to the realm of metaphysics and they are, therefore, useless to the scientist. Though they are useless as subjects of science, however, they are useful as objects of scientific research. A scientist does not use 'what ought' statements because they cannot help him/her to make predictions concerning 'what is' statements. Nevertheless, since normative statements are part of subjective reality they can be studied in a positive fashion (to the extent they are amenable to scientific observation). On the other hand, statements relating to the ways the ends should be accomplished, or to the effects of a change in these ends, are positive and refer to the way things actually 'are'. The allocation of scarce resources to alternative ends is a fixed rule applying to all cases and is factually true of any context or situation<sup>36</sup>. The study and explanation (prediction) of such rules constitutes the subject-matter

of Neoclassical economics. Predictions based upon such rules are synthetic statements that can easily be put into empirical test.

A development that grew out of this distinction was Marginalism and the consequent expansion of quantification<sup>37</sup>. Economics by defining its scope in terms of scarcity and the allocation of means to alternative ends reversed the old problems of Classical economics from value being determined by labour and production to value being determined by utilities and exchange<sup>38</sup>. In fact the term value was discarded and price (the result of exchange) took its place. Price was determined by the interaction of economic agents in the market. The old problems of distribution and accumulation became problems of exchange. Supply and demand became the cornerstone of economic science. Everything, therefore, was in accordance with the demands made by Positivism. Firstly, all action was conducted on the surface (exchange, market) and therefore could be empirically observed. Secondly, via Marginalism, mathematics could be applied and the world appeared - as in physics - as a set of mathematical functions and interrelations. Thirdly, price was a quantifiable concept and, more importantly, it rendered all other economic concepts quantifiable<sup>39</sup>. Fourthly, by virtue of putting boundaries on the subject of economics, the delineation of 'pure' relations made quantification and empirical observation easier<sup>40</sup>. And fifthly, the individualization of the subject of economics, along with the assumption of the selfish pursuit of interest, allowed the formation of utility or preference functions that could be ordered and therefore quantified.

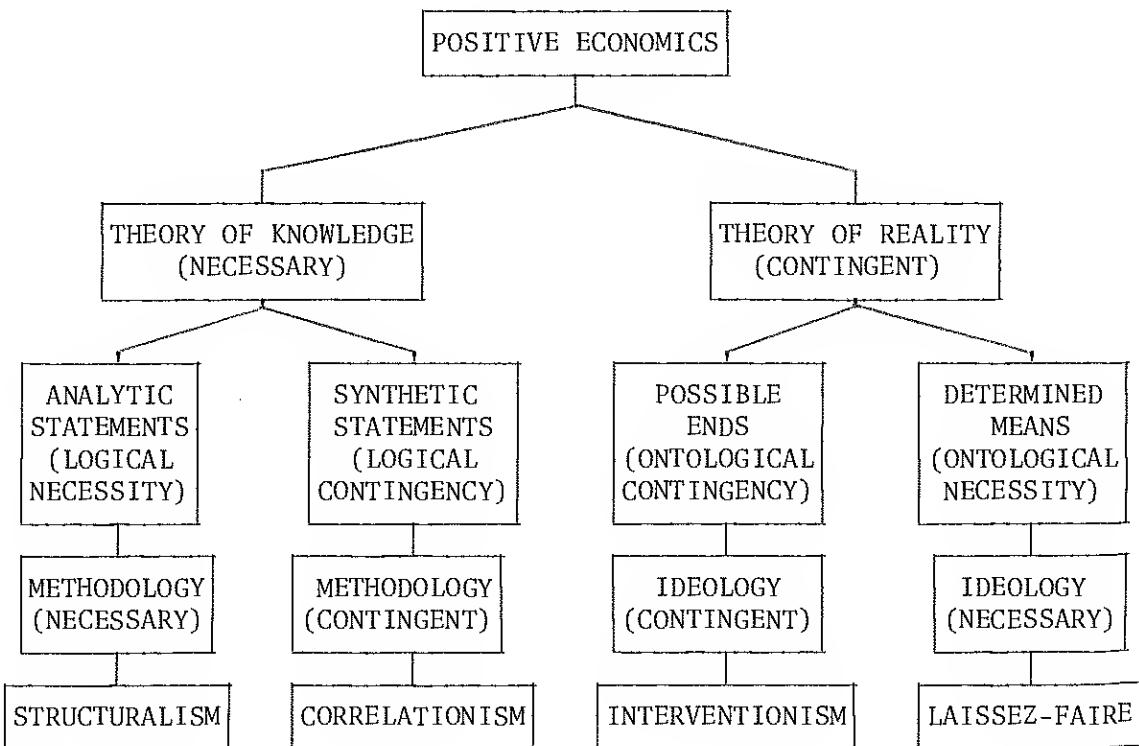
In consequence, economics acquired the status of science because it separated positive from normative statements, rendered its theories quantifiable, defined its empirical scope, induced measurement, and finally, by virtue of all this the fundamental tenet of Positivism was fulfilled: empirical testing and observation became possible and hence Political Economy turned into the science of Economics<sup>41</sup>.

#### a. NECESSITY AND CONTINGENCY

In continuing the logical synthesis between Positivism and Utilitarianism we see that the tension between knowledge and reality or between the implied necessity and contingency, forming the basis of Positivism and Utilitarianism, is repeated in their synthesis, namely in the structure of Positive economics. Figure VII picks up where Figure VI has left, and depicts the continuation of the tension in the structure of Positive economics. The epistemological part of Positive economics borrows directly from the positivist theory of knowledge and the behavioural (ontological) part directly from neo-classical theory. Each building block in Positivism and Neoclassical economics contains this tension in a complete form: necessary objectivity-contingent subjectivity in economics (see Figure VI). As a fusion between the two, however, Positive economics inherits the tension as a result of a transaction. From Positivism the necessity-contingency opposition is decomposed and Positive economics inherits the part relating to the necessary rules of science. The contingent object in Positivism is incorporated in the contingent part of Positive economics. From Neoclassical economics the equivalent tension is also decomposed

and the contingent subjectivity part becomes the world (reality) of Positive economics. The part relating to the necessary objectivity of the neoclassical theory of reality is reduced either to an 'as if' representation of reality or an 'ideal-type'. In essence, therefore, the neoclassical theory of reality is composed of a part referring to the actual state of the world (ends) and a part referring to an assumption about the world (means). In the synthesis of Positive economics the actual contingent part of neoclassical theory becomes the Positive economic theory of reality and the necessary (means, rationality) part is incorporated in the scientific rules taken from Positivism.

FIGURE VII: THE LOGIC AND STRUCTURE OF POSITIVE ECONOMICS



The necessary part relates to the scientific method used in economics. As in Positivism it provides a set of criteria by which the economist can choose between alternative theories. The assumptions behind these criteria are universal and fixed because they apply to any situation irrespective of its content. For example, the precept that a theory should systematically be put into empirical test, is a rule valid for any theory anywhere and anytime. The method by which one is to acquire scientific knowledge is independent of the phenomena under study or the theories interpreting them. One has to test theories with facts and facts should be empirically and quantifiably amenable. In this sense, therefore, a set of methodological rules becomes the necessary component of Positive economics.

In sharp contrast to the fixed criteria for knowledge, Positive economics believes that the world is too complex and the events therein occurring not in any prearranged way but unnecessarily. There is no necessary connection between any two events. There are only systematic regularities. Following a strongly empiricist tradition Positive economics considers the world as one among possible others, one that could be otherwise<sup>42</sup>. The economist cannot know the world a priori, he/she has to experience it. The theory of reality espoused in Positive economics is, therefore, a theory of contingent reality<sup>43</sup>.

However, in further analysing the methodological (knowledge) and behavioural (reality) premises of Positive economics, we see that the necessity-contingency tension reasserts itself in a different form,

i.e. that of logical necessity versus logical contingency and that of ontological necessity versus ontological contingency (see Figure VII).

The first one manifests itself in the distinction between analytic and synthetic. This distinction is indispensable because Positive economists acknowledge the existence and need of quasi a priori statements or theories (as we have seen in chapter 1). These are used either to set an ordered perspective to the mass of unordered data and to render complex reality intelligible, or to be used as 'as if' theories. Maximization and perfect competition are two examples of analytic statements. These statements are normally defined as a set of tautologies that are empirically meaningless. As such their inner content is logically necessary. If, for instance, we define  $MV=PT$  to be such so as when we keep velocity and output constant, then necessarily money will equal the price level. Or if all the conditions of the perfect competition model hold then necessarily the producer will produce that output which will maximize profits. Since the conclusions follow directly from the premises - in fact they are contained in the premises - they are therefore logically necessary. (It should be noted, however, that although analytic statements are logically necessary they are nonetheless epistemologically contingent. That is, all analytic statements are considered heuristic or ideal-type constructs the choice of which partly depends on subjective criteria as well as criteria such as simplicity or fruitfulness<sup>44</sup>. Their status is tentative even if they are confirmed by experience. Positive economists believe that

to a single phenomenon there may be an infinite number of interpretations or theories. A priori choosing between alternative interpretations has to be based on epistemologically contingent criteria.)

The opposite of logical necessity is implied in the formulation of synthetic statements. By contrast to analytic statements, the elements comprising synthetic statements are not abstract or vacuous but represent concrete, empirical reality. For example, the statement that a change in the money supply will induce an upward (downward) shift in income is supposed to be a synthetic statement the validity of which depends on whether a measure of money systematically leads (lags) a measure for the price level, both quantities measuring empirical variables. Since the nature of synthetic statements is empirical, inferences cannot be drawn with a logical method, as in analytic statements. They are not logical constructs the conclusions of which are necessarily implied in the assumptions. From the point of view of logic they are contingent. They depend upon empirical reality which is contingent and unnecessary. They are therefore logically contingent. (From an epistemological point of view, however, they are necessary. Their choice does not depend upon the subjective state of the individual scientist, but upon the necessary requirements and rules of observation and experiment.)

The second tension, i.e. that of ontological necessity versus ontological contingency, manifests itself in the equally indispensable distinction between possible ends and determined means.

Ends reflect the subjective part of the world, which should be accepted as given and unquestionable, while means reflect the objective part of the world, the domain to which the economist should be restricted. (See Figure VII.)

On the one hand the nature of ends is not considered fixed. Ends belong to a subjective realm where everything is possible. Every end is a possible one and could be otherwise. A consumer may value the acquisition of luxury cars or the purchasing of stamps. Each end belongs to one's subjective taste or psychology and is not supposed to be scientifically questioned. National governments may attempt to fulfil different ends based upon different priorities or ideologies. A socialist country, for example, may value more the expansion of heavy industry or a more equal distribution of income, whereas a capitalist country may value more the expansion of consumer goods and service industries and a more efficient (and unequal) allocation of resources. The possible ends that people or countries can have are almost infinite and contingent upon a variety of factors. Since this tenet of Positive economics refers to actual reality its characteristic is, therefore, that of ontological contingency.

On the other hand, the ways or the means with which individuals or societies fulfil this variety of ends are determined. They are determined by virtue of scarcity and the wish to allocate rationally and optimally. Irrespective of whether a rich man wants to buy a yacht or a poor man wants to buy a loaf of bread, each one will attempt to attain these ends according to the same principle,

i.e. each one will allocate his/her finite income in such a way as to acquire the maximum utility with the minimum cost. The principle is universal and applies equally to planned and 'free' economies<sup>45</sup>. Although maximization is considered an ideal-type it nevertheless refers to a representative tendency among social agents which determines their actual behaviour. Accordingly, as the nature of means is assumed necessary, and as they refer to the real world, they imply ontological necessity.

The above distinctions between knowledge and reality, analytic and synthetic, ends and means, disparate though they seem, are characterized by a structured correlation which holds together the totality of the Positive economic system. The binding force which pieces together the structural components of Positive economics lies in the ubiquitous wish to limit what is scientifically viable and what is not. That is, in the wish to resolve the Problems of Knowledge and act 'scientifically'. The reaction against older metaphysics and authority led to the opposite, namely a reverence for what is objective and free. In the case of methodology Positive economics defines its assumptions for the criteria of choosing between alternative theories, not according to some metaphysic but according to empirical facts. Facts, neither man nor God, should decide which is the true theory. In the case of actual behaviour Positive economics defines its assumptions for the criteria for choosing between alternative ends, not according to the laws of external authority but according to universal rules that maximize efficiency.

b. METHODOLOGY AND IDEOLOGY

The cost, however, of establishing such assumptions is the ever-present tension between necessity and contingency. This is further repeated at the methodological and ideological levels of Positive economics (see Figure VII).

At the methodological level it manifests itself in the formation of conflicting methodological schools. These schools are the Structuralists<sup>46</sup> (Assumptionists) and the Correlationists (Predictionists). The Structuralists correspond to the school that espouses the assumption of a complex reality but sets as its aim the attempt to model this reality accurately and descriptively. In addition Structuralists (as Assumptionists) claim that validation should be done more at the level of assumptions rather than only at the level of predictions. Although their method admits only synthetic statements as the final arbiter of competing theories, it also accepts analytic statements and the study of factors that faithfully reflect the economic complexity and which are factually and logically interrelated.

On the other side the Correlationists start from the same premise of a complex world and conclude that, since the world is much too complex its interrelations cannot be adequately traced out. Therefore, one should abstract from them in order to be able to construct rigorous theories. The objective of the Correlationists (as Predictionists) is to formulate simple hypotheses that can generate empirical predictions and test their validity with synthetic statements.

Their job, therefore, is to trace regularities, explain them with a few variables and test their explanatory power with the systematic application of their predictions with experience<sup>47</sup>.

As noted in previous chapters, however, economists are not separated as Structuralists or Correlationists in any clear-cut manner. These labels pertain to opposing tendencies existing simultaneously within Positive economics. This is mainly shown from the fact that both schools adhere to the principle of empirical validation and accept the distinction between positive and normative statements (see chapter 1). A methodological conflict within Positive economics is possible because it stems from a tension that is structurally built in its epistemological premises. This tension, which is caused by the ambiguities discussed in chapters 1 and 2, reappears in the distinction between analytic and synthetic statements permitting the polarization of interpretations as to the appropriate nature of method.

On the one hand, the presence and function of analytic statements, implying logical necessity, allows the economist to accept a methodology that tackles complex reality in a comprehensive and analytic way, portraying all the logical and factual connections. Structuralists rely on one foot of Positive economics, as it were, and stress the importance of logically analysing the world. That is, their methodological premise is the continuous accumulation of analytic statements, in the form of complex, structural models, that enable them to come closer to the synthetic truth, i.e. approach empirical reality by persistently analysing its structure. Moreover by doing this they

do not break any positivist rule since their method can always be justified by an appeal to the structure of Positive economics itself and especially to its analytic part.

On the other hand, Correlationists base their methodological claims on the other foot of Positive economics, i.e. synthetic statements. The logical contingency implied induces economists to decide that they should rely on simple synthetic hypotheses upon which to base their knowledge of the world. In fact, it is believed that the accumulation of such synthetic hypotheses will lead them to the acquisition of synthetic truth. Moreover they too feel secure that Positive economics will endorse their claims, since synthetic statements are part of its structure.

Hence, the methodological dichotomy splitting Positive economics is stimulated by the way the theory of knowledge in Positive economics is structured<sup>48</sup>. This methodological conflict is exemplified in the Monetary and F-Twist controversies. In the one case, the fight is between structural models and single-equation tests, and in the other, between Assumptionists and Predictionists. Both factions claim to be scientific and both factions justify their methodological claims by appealing to Positive economics. Having said this, the question arises, however, as to what it is that makes certain economists espouse one interpretation of Positive economics and other economists the other? The answer, as we shall see, lies in the interrelation between methodology and ideology.

At the ideological level the necessity-contingency opposition, found at the methodological level, appears in a transposed form. It appears in the distinction between ends and means and it permits interpretations of reality that are conflicting. This is manifested in the conflict between economists supporting interventionism and economists supporting laissez-faire<sup>49</sup>, (see Fig.VII).

On the one hand, in the case of Interventionism, the multiplicity of possible, contingent, ends sets out a perspective through which the economist views the world as a set of interrelated sectors the complexity of which is considered enormous. Each of these sectors contains an almost infinite array of ends that need to be realised. It is believed that social agents in their attempt to fulfil their objectives within each sector, are confronted with all sort of lacks and rigidities, due to the institutional complexity. This is believed to hinder their actions in applying maximization rules in order to accomplish their ends optimally. Their action, although rational, does not lead the economy into general equilibrium. The Keynesian under full-employment equilibrium is very much a case in point. Over and above this, it is believed that in modern economies ends among and within sectors may conflict<sup>50</sup>. It is thought that private welfare does not necessarily lead to social welfare. Pollution, for example, does not - at least in the short-run - impede the maximization of private returns, whereas it impedes the maximization of social returns. Education and health are two more instances of a clash between the public and the private sector. Consequently, rigidities or conflicts between ends among and within sectors

necessitate the 'discrete' intervention of an organization that is, presumably, above all sectors and which is able to coordinate and integrate the actions of all sectors. Hence the need for a 'mixed economy' with a central government acting through direct (nationalization, planning) or indirect (taxes, transfers) ways to coordinate the alternative and conflicting goals in a strategy that renders the function of the economy optimal and leads to equilibrium<sup>51</sup>. The interventionist ideology is based upon the contingent ends assumption and forms a modified Neoclassical economics: a Neo-Neoclassical synthesis.

On the other hand, in the case of laissez-faire, there is a strongly held belief that the market will automatically adjust disequilibrium states if left to its own, while the price system, through competition, will dissolve any rigidities and conflicts by allocating scarce resources optimally<sup>52</sup>. This belief is primarily based upon the 'means' part of Neoclassical economics. The market mechanism is considered the natural way to apply the necessary allocatory rules to maximize resources efficiently. Any sort of outside intervention is considered a second best solution leading away from the Pareto optimal position. Inherent in this belief is the assumption of rationality. Man is believed to be more or less economically rational and if left to act freely, i.e. unhampered by any externally superimposed authority, will always try to optimize his scarce means to accomplish his private ends. If everyone in the economy acts in the same manner, the allocation of total resources is also optimized. For example, if pollution starts to become a cost to society eventually

the industry creating it will find it necessary, in the long-run, to internalize the costs of pollution as it gradually becomes an external diseconomy. Consequently, in its effort to minimize these costs it also automatically minimizes pollution. It is believed that for any problem, conflict or rigidity in an economy, the price mechanism (if well-oiled, i.e. three to four per cent of money growth) will necessarily lead to the best fulfilment of the desired ends. Or put differently, once economic agents make their choices rationally and allocate their resources optimally, then it follows that the sum total of the agents, i.e. society, will also be at an optimal point<sup>53</sup>. The ideology of laissez-faire rests its case upon that part of Positive economics emphasizing the rules that necessarily allocate resources optimally.

The proponents of either belief, however, will probably never admit that their views are ideological<sup>54</sup>. They are Positive economists and as such their assumptions about the world are so either because the world is so or that it is as if so. Yet it cannot be argued that these beliefs were found simultaneously with the development of Positive economics, thus creating the impression that they are part of the reality of the modern world. They existed long before and independently of modern economics<sup>55</sup>. Laissez-faire grew with Mercantilism, long before the 19th century, as an ideology viewing the role of man as an individual acting rationally in a complex environment. Interventionism is also an ideology developed by the early socialists and Utopians who viewed the role of man as too weak in relation to the complexity of the world needing, therefore, help. The integration

and refinement (or spurious concealment) of these ideologies within Positive economics created two poles that manifest themselves in the distinction between ends and means. The interpretation - or perhaps exaggeration - of these poles permits a conflict of ideologies that is concealed under the securing, scientific, blanket of Positive economics<sup>56</sup>.

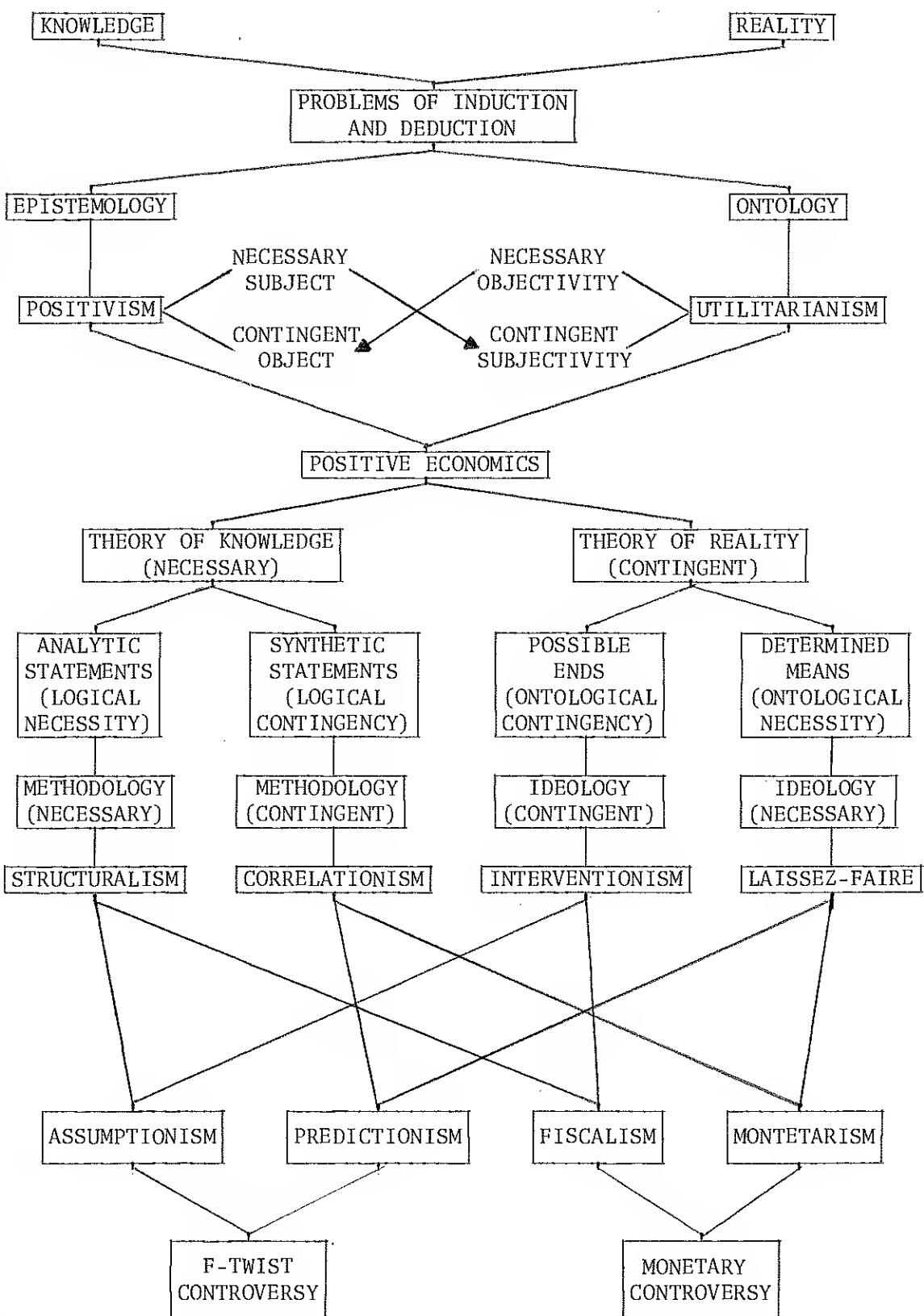
Having accounted for the ideological part of Positive economics we are now in a position to understand why some economists have chosen one methodology instead of another, why Structuralism instead of Correlationism or vice versa. The answer again lies in the necessity-contingency opposition. In the ideological part of Positive economics, economists are dichotomized between a theory of a necessary and a theory of a contingent world. The first one relates to laissez-faire and the second to interventionism. As it stands this opposition needs to find its counterpart in a theory of knowledge in order to justify its claims to knowledge. In other words, since each ideology cannot find its logical correlate in the other ideology it looks for it in the methodology part of Positive economics. The contingent world of Interventionism allies with the necessary methodology of Structuralism in order to form the structured opposition between logical necessity and ontological contingency. The contingent world of interventionism can be scientifically assessed by the necessary logic of a structuralist methodology. On the other hand, the necessary world of laissez-faire finds its natural ally in the contingent methodology of Correlationism. The juxtaposition between logical contingency and ontological necessity is formed again as a

transformation of the opposition between logical necessity and ontological contingency. This cross-exchange forms a dialectical process in which the choice of one particular ideology or methodology determines its counterpart. The nature of ideology is structurally combined with the opposite nature of methodology and vice versa. An Interventionist, for example, could not ally with a correlationist methodology as this would go against his/her fundamental assumption of the nature of the world being made of an uncoordinated multiplicity of states. Equally an adamant supporter of the efficacy of the market system working automatically could not ally with Structuralism as this would go against his/her assumption of a well structured and fundamentally simple mechanism operating amidst the complexity of events. Thus, there is no one-way causal direction from ideology to methodology or from methodology to ideology. The process is dialectical rather than causal. Just as the ideology, i.e. the theory about reality, specifies the way with which to know reality, so also the methodology, i.e. the theory about the way to know reality, specifies the form that reality takes<sup>57</sup>. What connects the opposing terms is the specific logic manifested in the structural relation between necessity and contingency.

The synthesis between Interventionism and Structuralism constitutes the foundation of Fiscalism and correspondingly the synthesis between laissez-faire and Correlationism constitutes the foundation of Monetarism. These syntheses also constitute the foundations of Assumptionists and Predictionists. The opposition between Fiscalism and Monetarism forms the Monetary controversy and the

opposition between Assumptionism and Predictionism forms the F-Twist controversy. The interrelations that lead to these conflicts are depicted in Figure VII (on p.330) whereby the structure of these conflicts is delineated. Figure VIII puts together all the previous arguments and, as if in a jigsaw puzzle, fits the logical pieces for an explanation of conflict in economics. In other words, the distinctions and categories of Positive economics are put in relation to one another according to the structural relation of necessity-contingency. This structural relation appears in various combinations forming a deeper structure logically binding the distinctions of Positive economics. This 'complex matrix' of relations also constitutes the basis for persistent conflict. The diagram in Figure VII can be read from either end. If we start from the top end we have the oppositions within the structure of Positive economics stemming from the synthesis between Positivism and Utilitarianism, that lead to conflict. If we start from the bottom end we have two controversies that can be explained in terms of the oppositions derived from their inner logic. On the one hand, Positivism by attempting to discover reality stumbles upon the Problems of Induction and Deduction and thus, through mediations, combinations and distinctions, creates a school of thought, namely Positive economics, that is riddled with ambiguities and tensions which lead to permanent conflict. On the other hand, conflict in economics, or the logic of it, can be decomposed to its constituents that lead in a structural way to the basic oppositions necessitated by the fundamental ambiguities in the positivist attempt to know and discover reality. In either case the structure and logic of Positive economics is analysed and an understanding of perpetual conflict in it becomes possible.

FIGURE VIII: A RECONSTRUCTION OF THE LOGIC AND STRUCTURE OF CONFLICT IN POSITIVE ECONOMICS



## D. CONCLUSION

Thus, the continuing persistence of the F-Twist and Monetary conflicts can be explained in terms of more fundamental conflicts in Positive economics. These conflicts are structural in the sense that each part of a conflicting relationship cannot stand logically on its own but needs the other part to form a whole. The tension that is generated stems from the striving of each part of the conflict to achieve a logical identity with its complement. The identity of each part depends on its relation to other parts in the system. Each part forms a member of a dichotomy or conflict that needs its opposite to form a logical and coherent unity. As we have seen the structure of Positive economics consists of such dichotomies and conflicts that are united by the opposition between necessity and contingency. In fact controversies in Positive economics are both a reflection and a manifestation of these conflicts. The logical oppositions binding the structure of Positive economics form a deeper structure that is behind the ideals and distinctions of Positive economics. These logical oppositions through a series of transformations manifest themselves in the surface structure of Positive economics in the form of persistent conflicts, such as the Monetary and F-Twist controversies.

Although Monetarists and Fiscalists think that they are separated only by theoretical and empirical issues, their divergence is much wider and deeper, embracing methodological (epistemological) and ideological (ontological) differences. However, it is accepted by all sides that the issues can be easily decided with an appeal to empirical facts. Yet the appeal has been made and the verdict still

remains ambivalent. Both Fiscalists and Monetarists find their position justified by the same judge: empirical facts. Both sides being parts of Positive economics hold as their prime target the confrontation of theories with empirical experience. Both sides, however, having found justice in empirical experience, resort to methodological and theoretical arguments to further justify or qualify their position. And indeed these arguments and qualifications are found in the basic dichotomies inherent in the structure of Positive economics. Each faction emphasizes a particular part of this structure and rationalizes its position in relation to it. The structured dichotomies, because they are the outcome of fundamental ambiguities and unresolved tensions, nurture the development of conflicting ideas and methods. The irony is that the same paradigm gives an epistemological solution that is meant to solve the conflict, namely empirical testing which is, however, found within the structure itself that has created the conflict. The conflicts persist because the paradigm that nurtures them gives them also their justification. In other words, both sides in the controversies find epistemological and ontological justification in the distinctions of Positive economics, the same distinctions that have caused their emergence.

## C O N C L U S I O N

## C O N C L U S I O N

The general purpose of this thesis has been to examine the logic of persistent conflict in economics. Thus no effort was devoted to a direct critique of the methodology of Positive economics, but rather to an account of the reasons that make it self-defeating. The objective was not to take each part of Positive economics and subject it to a critique of its defects, but present the whole structure of Positive economics in a light that exposes the mechanism which renders it ineffective. The case of the Monetary controversy has been chosen as a typical example of this ineffectiveness. It should be noted, however, that the event of empirical testing not working in the case of the Monetary controversy is by no means conclusive evidence of empirical testing not working in general. To prove this one would need a systematic study of many controversies where empirical testing is involved. Yet, from a logical point of view, Positive economics is committed to an uncompromising pledge that its methodology, given appropriate methods and data, would discriminate between all conflicting theories. Had it not been so, Positive economics would not have been able to apply logical criteria for showing which set of conflicting theories it can resolve and which it cannot. Hence, according to this logic, it suffices to find one counter-instance falsifying the methodological hypothesis of empirical testing in order to show that it is not a necessary condition for theory validation.

Thus, the main question put forward in this thesis was: What is it that makes the methodology of Positive economics persistently inoperative in a case such as the Monetary controversy?

During the course of this research it eventually became clear that in order to answer this question I should not regard it from an alternative methodology's point of view (*viz.* Hollis and Nell) but from within Positive economics. The reasons were that although a critique of Positive economics from without could provide a sufficient explanation of the persistence of the Monetary controversy (such as for example that the theories in question are not testable, or that ceteris paribus clauses are elastic, or because of errors in data, definitional problems and ideological bias) a structural analysis of the methodology of Positive economics from within would provide a necessary explanation. The explanation would be necessary because it would expose the logical mechanism within Positive economics which necessitates the recurrence of the bi-polar situation from which persistent conflicting ideas about theory and method stem.

The fundamental assumption behind this explanation was that problems in the theory of knowledge remain largely unresolved and that therefore no epistemological system is unequivocal. However, since the decision has been taken to know the world (be it economic or otherwise), concessions, assumptions, distinctions and mediations had to be made in order to circumvent the Problems of Knowledge, e.g. the Problems of Induction and Deduction, the Problem of Demarcation Criteria, etc. (For instance in Physics the assumption of the uniformity of nature - itself creating a logical regress - mediates for the irresolution of the Problem of Induction. Even if the Problem remains unresolved, the assumption, as long as it actually works in practice, permits the use of inductive inference. However,

as far as economics is concerned the assumption of the uniformity of society would hardly apply<sup>58</sup>.) The contradiction is as follows: People (scientists and lay-men alike) need to know and therefore act in a specific manner in order to know. In the past an axiomatic and necessary authority provided the source of all knowledge. No logical justification of its origin was asked for. However, as soon as 'enlightened rationality' took the place of the 'superimposed ideology'<sup>59</sup>, people were led to justify the source of their knowledge in a logical way. But as soon as they were confronted with the unsolvable problems of knowledge logic (and rationality) deserted them. Thus, if people claim to be rational and face such a logical impasse, then they must stop actively seeking knowledge, since they cannot justify this action. Yet they go on acting thus, despite the logical contradiction between the rational justification of their action and their assumption of rationality.

This general contradiction about knowledge and its justification is transferred to economics and runs through its constituents. The application of inductive methods to economic phenomena carries with it the logical paradox of the Problem of Induction. More than any other epistemological system, Positive economics professes to be rational (in the sense of Comte and the Vienna Circle) and yet cannot offer a rational justification of the ways it extracts economic knowledge. This logical impasse, however, is mediated (or perhaps repressed) with distinctions and demarcations that seem to offer a viable methodology. This seeming viability, nonetheless, is achieved at the cost of a structured ambiguity, which in the form of a tension runs through the distinctions and the

demarcations. This latent tension takes the form of a contingency-necessity structural relation. The permeation of this tension throughout the structure of Positive economics creates a polarity in each concept that is the result of the general ambiguity between economic knowledge and reality. This binary nature of the concepts of Positive economics is homologous to conflicting ideological views about the nature of the economy and the way to know it. Hence, for example, the genesis and continuation of the Monetary and F-Twist controversies<sup>60</sup>.

Thus, a fundamental ambiguity manifests itself as an antinomy or tension that polarizes the concepts of Positive economics. This polarization permits conflicting interpretations of the epistemology and ontology of Positive economics. These interpretations, in turn, nurture the development of controversies. These controversies persist because the methodology that is presumed to resolve them is produced from the same ambiguous paradigm that creates them.

It is obvious, however, that an ambiguity runs through the assumption behind the above explanation, in that although I proclaim a scepticism that seems to leave no alternative open for any kind of discussion (scientific or otherwise), implicitly I offer scepticism as an alternative, which if it would have to be consistent should also negate itself. I feel that I have set up a trap in which I have fallen myself. Yet I have fallen in the trap voluntarily when I decided to offer an explanation of conflict in economics. The ambiguity even permeates the idea of writing this thesis itself.

If, in accordance with my basic assumption, everything is logically fluid and there are no foundations - epistemological or ideological - to support any position, then the writing of the thesis itself is based on fluid foundations which make it a very shaky construct.

But why then do I write a thesis when I know that the idea is ambiguous? The reason is concessionary. I do it because I want (need) to explain persistent conflict in economics. The urge to know is equally strong as the urge to be logically consistent. In the conflict between knowledge and logical consistency no side ever takes the upper hand. The crippling effects of the philosophical regresses are countered by the attempt to know and explain. Although one knows that one's assumptions would be riddled with ambiguities one goes on to find ways to bring things into awareness. One of the main reasons, therefore, for writing this thesis was to bring out on the surface the tangled threads of the logical structure of Positive economics in order to show the source of conflict within it. Despite the logical impasses and ambiguities inherent in the assumptions behind the writing of this thesis it was felt imperative and worth while to search for new (but perhaps ambiguous) knowledge.

Thus my justification for writing this thesis contains a structured ambiguity (fluidity) that is mediated by the need to know and explain, in order to produce a solution and make the writing of the thesis viable. As in all cases, so in this, "behind all sense there is a nonsense"<sup>61</sup>.

## FOOTNOTES

## PART III: CHAPTER 7:

1. "The Political Element . . .", op.cit., p.36.
2. "Economic Philosophy", op.cit., p.10 and p.28.
3. Le Myth de Sisyphe, 1942, p.26.
4. "Introduction", in "Value in Social Theory", op.cit., p.X.
5. By 'onto-logical' I mean, in a specific sense, the attempt to explain on the one hand, the logical interconnections of a system and on the other, to show how these interconnections are structured in such a way as to form the actual foundations of its manifold complexity and of conflict in it. I use the literal meaning of 'ontological': 'on' = being, actual, and 'logos' = logical (see the Concise Oxford Dictionary, Fifth edition, p.846). Later on in this chapter I will use 'ontological' in the conventional sense, i.e. something that refers to actual existence or being.
6. B. Russell, History of Western Philosophy, 1961, esp. pp.447-454, pp.479-482 and pp.512-525.
7. Boulding, "Economics As A Moral Science", op.cit., p.1.
8. Russell, ibid., p.1.
9. Hayek, 1942, pp.270-271.
10. See Kolakowski, "Positivist Philosophy", op.cit., p.11.
11. ibid., p.12.
12. ibid., p.208.
13. Hollis and Nell, "Rational Economic Man", op.cit., p.4.
14. For instance Streeten says that, "The philosophy which denies the logical connection between facts and values and deduces from this denial its own moral neutrality . . . suits admirably a liberal philosophy of tolerance", "Introduction", op.cit., p.X]III, see also p.X IV.
15. ". . . the traditional positivist attitude . . . recognizes only those human effects measurable by utilitarian success and ascribes the dignity of knowledge only to that with which 'you can do something'", Kolakowski, ibid., p.234.
16. See, e.g. W.J. Samuels, "The History of Economic Thought as Intellectual History", History of Political Economy, 1974, pp.308-309.

## FOOTNOTES (cont.)

## PART III: CHAPTER 7:

17. "Rational Economic Man", op.cit., p.48.
18. J. Robinson, "The Theory of Value Reconsidered", in her Collected Economic Papers, 1973, p.61.
19. R. Meek, "The Marginal Revolution and Its Aftermath", in Hunt and Schwartz, "A Critique of Economic Theory", op.cit., p.97. See also his "Economics and Ideology", op.cit., p.209.
20. Quoted in Hutchison, "'Positive' Economics and Policy Objectives", op.cit., p.38.
21. "The rise of the marginal utility theory of value, and its successor, the analysis of consumer's preference and choice, with their emphasis on the subjectiveness of values and valuations, encouraged in a superficial way the view that social values, the ends of policy, and value-judgements generally, were relative and subjective, as contrasted with the 'objective', 'positive' propositions of economic science", Hutchison, "Positive Economics . . .", op.cit., p.40.
22. See Robbins, "An Essay on the Nature and Significance of Economic Science", op.cit., pp.18-24.
23. See S. Latsis, "Situational Determinism in Economics", British Journal of the Philosophy of Science, 1972, p.204.
24. "Scarcity of means to satisfy ends of varying importance is an almost ubiquitous condition of human behaviour", Robbins, ibid., p.15. "Anglo-saxon economics . . . rationalized the market mechanism as the necessary vehicle to eventuate the good society", D.A. Martin, "Beyond Positive Economics, Toward Moral Philosophy", American Economist, 1973, p.62.
25. For this combination and the development of Utilitarianism see Myrdal, "The Political Element . . .", op.cit., pp.34-36.
26. As Walras says, "the pure theory of economics ought to take over from experience certain type concepts, . . . the pure science of economics should then abstract and define ideal-type concepts in terms of which it carries on its reasoning", "Elements of Pure Economics", 1954, p.71. See also Hutchison, "Positive Economics . . .", op.cit., p.24.
27. As a theory of knowledge standing by itself Positivism contains the necessary ingredients in its structure that form a complete system. The contingent world is counteracted by the necessary scientific rules. The same applies to Neoclassical economics. The disparity of values (ideologies) and ends is ordered with the introduction of scarcity and rationality. However, in forming a synthesis in Positive economics, the positivist general world is replaced by a world defined within an economic system.

(cont.)

## FOOTNOTES (cont.)

## PART III: CHAPTER 7:

27. (cont.) Correspondingly, a purely economic theory of reality needs an epistemology to render its system complete. Although the distinctions in the one system correlate with the distinctions in the other, i.e. necessary rules of knowledge relate to necessary rules of conduct, and contingent general world with contingent economic world, the Positive economic synthesis is realized - to form a complete system - only with a cross-exchange between a theory of knowledge and a theory of economic reality.
28. The arrows in Figure VI do not denote causation but rather show the direction of cross-exchange.
29. See, e.g. H. Sidgwick, "The Scope and Method of Economic Science", in Smyth's, Essays in Economic Method, 1962, p.97.
30. Samuelson, "Economic Theory and Mathematics . . .", op.cit., p.47.
31. J.S. Mill, "On the Definition of Political Economy; and the Method of Investigation Proper to It", in his, "Logic of the Moral Sciences", op.cit., p.124.
32. See, e.g. Marshall, "Economics of Industry", op.cit., pp.19-22. Also Heilbroner, "On the Limited Relevance . . .", op.cit., pp.84-87, and esp. Rothenberg, "Values and Value Theory in Economics", op.cit., pp.224-227.
33. See D. Winch, "Marginalism and the Boundaries of Economic Science", History of Political Economy, 1972, p.337. Although Hayek criticizes the methodology of Positivism, he nevertheless directs his attack of 'wholism' against Marxism and Historicism rather than Positivism.
34. This alliance, however, should not be seen in terms of two separate schools coming together, but rather in terms of a parallel movement between Positivism and Utilitarianism, each one shaping and affecting the other.
35. See Hutchison, "Positive Economics and Policy Objective", op.cit., pp.15-42. Also Hill says that "As a denial of normative value, positive economics distinguishes between the means and ends of the economic process and denies the economist the privilege of justifying economic ends", "A Critique of Positive Economics", op.cit., p.263. Finally, Streeten claims that, "The means-ends model was constructed in order to salvage something of what appeared to be a normative science, in an age when the rationalistic faith in discoverable values had declined, and scepticism and relativism had grown stronger", "Introduction", op.cit., p.XIV.
36. See Robbins, "An Essay . . .", op.cit., pp.15-18.

## FOOTNOTES (cont.)

## PART III: CHAPTER 7:

37. See M. Dobb, "The Trend of Modern Economics", in Hunt and Schwartz, "A Critique . . .", op.cit., pp.39-80, N. Bukharin, The Economic Theory of the Leisure Class, 1927, pp.15-46, Schumpeter, "History of Economic Analysis", op.cit., pp.909-920, A.W. Coats, "The Economic and Social Context of the Marginal Revolution of the 1870s", History of Political Economy, 1972, pp.303-324.
38. ". . . the formula of the English school, in any case the school of Ricardo and Mill, must be reversed, for the prices of productive services are determined by the prices of their products and not the other way around", Walras, "Elements . . .", op.cit., p.45. See also Myrdal, "The Political Element . . .", op.cit., p.221.
39. Marshall, "Economics of Industry", op.cit., p.20.
40. Meek, "Economics and Ideology", op.cit., p.206.
41. On the unification between formalism and Positivism in economics see Ward, "What's Wrong with Economics", op.cit., pp.143-146.
42. See Hollis and Nell, "Rational Economic Man", op.cit., p.4.
43. "The relations between the events which constitute the raw material of economics are not constant and immutable. They are relations of partial dependence, of contingency", Mills, "On Measurement in Economics", op.cit., p.48, see also pp.40-41 and p.49. Cf. E. Rotwein, "Empiricism and Economic Method", Journal of Economic Issues, 1973, pp.363-364.
44. See Friedman, "On the Methodology . . .", op.cit., p.8.
45. Robbins, "The Nature and Significance . . .", op.cit., pp.15-16.
46. Structuralists here should not be confused with structuralists of the Lévi-Strauss or Chomsky kind. I use the term to convey the idea of the fiscalist methodology of building large-scale models portraying the full structure of the economy.
47. For a description of the two schools see Bronfenbrenner, "A 'Middlebrow' Introduction . . .", op.cit., pp.15-17.
48. ". . . the issue as between operationalism and realism is whether the statements connecting theoretical terms to empirical ones are to be regarded as analytic or synthetic", Coddington, "Positive Economics", op.cit., p.6.
49. According to Coddington ("Rethinking Economic Policy", op.cit., p.436), Neoclassical as opposed to Keynesian economics represents a different "style" of intervention and therefore is not laissez-faire proper. However, since the dichotomy between 'weak' and 'strong' intervention holds true, for the purposes of this chapter I will retain the terms laissez-faire and intervention. See also Culbertson, "Macroeconomic Theory . . .", op.cit., pp.537-538.

## FOOTNOTES (cont.)

## PART III: CHAPTER 7:

50. See, e.g. N. Kaldor, "Conflicts in Policy Objectives", in Policy Objectives, 1971, pp.1-19, or Knight, who remarks that, "A social problem arises out of conflicts of private ends, and some objective comparison is obviously necessary to any adjudication", "Institutionalism and Empiricism in Economics", op.cit., p.51.
51. Samuelson, "Economics", op.cit., pp.41-55. Also W.W. Heller, Perspectives on Economic Growth, 1968, pp.1-24. "in the absence of such (well designed discretionary) policies the system may drift away from the desired growth path or exhibit cycles undesirably large in amplitude . . .", op.cit., p.560.
52. M. Friedman, Capitalism and Freedom, 1962, pp.1-37.
53. Whereas, "increasing volatility and increasing government intervention with the price system are the major factors that seem likely to raise unemployment", M. Friedman, Inflation and Unemployment: The New Dimension of Politics, The 1976 Nobel Lecture, The Institute of Economic Affairs, Occasional Paper, 51, 1977, p.28. ". . . [the] basic conception of laissez-faire as an autonomous self-regulating system which, through the laws of supply and demand, produced an inevitable harmony of interests", P. Adelman, "Frederic Harrison and the 'Positivist' Attack on Orthodox Political Economy", History of Political Economy, 1971, p.170.
54. 'brute experience proved far more potent than the strongest of political or ideological preferences', Friedman, ibid., p.31.
55. See J.E. Elliot and J. Cownie, Competing Philosophies in American Political Economics, 1975, pp.1-29, and Myrdal who says that, "We must look upon the majority of modern economic doctrines as modified reminiscences of very old political thinking", "The Political Element . . .", op.cit., p.X. Also M.L. Myers, "Philosophical Anticipations of Laissez-Faire", History of Political Economy, 1972, pp.162-175.
56. For the relationship between modern economics and older systems see Martin, "Beyond Positive Economics: Toward Moral Philosophy", op.cit., pp.61-69, B.B. Seligman, "Philosophical Perceptions in Economic Thought", Journal of Economic Issues, 1971, pp.1-24, and Myrdal, "The Political Element . . .", op.cit., p.X.
57. "Theory is essential to practice . . . as well as to the practice it is the theory of", L. Althusser, "On the Materialist Dialectic", in his, For Marx, 1969, p.166.

## FOOTNOTES (cont.)

## PART III: CONCLUSION

58. See Culbertson, "Macroeconomic Theory . . .", op.cit., pp.114-115.
59. See J. Habermas, "Science and Technology as Ideology", in B. Barnes, Sociology of Science, 1972, p.361.
60. It seems prima facie plausible, however, that other conflicts within Positive economics, such as the ones concerning the Phillips curve, or the investment and consumption functions, may be studied within the same or similar framework of the explanation given in chapter 7.
61. C. Lévi-Strauss, "Responses à quelques questions", Esprit, November 1963, p.637.

A P P E N D I X

R A T I O N A L   S C I E N T I F I C   M A N

## APPENDIX

RATIONAL SCIENTIFIC MAN<sup>1</sup>

## A. INTRODUCTION

I have chosen to comment critically on Hollis and Nell's "Rational Economic Man" mainly for two reasons. Firstly, they offer a critique of Positive economics that is perhaps the most up to date and complete analysis of economic orthodoxy. And secondly, they suggest a strategy for an alternative framework which, according to them, claims to be free of the contradictions found in Positive economics.

Their critique is based on the epistemological insufficiencies of Positive economics and it is, partly, a result of their alternative framework, namely Rationalism. According to Hollis and Nell their "critique of Neoclassicism finally rests not on (their) critique of Positivism but on an alternative to it, Rationalism"<sup>2</sup>. In reviewing Hollis and Nell's critique of Positive economics my purpose is to juxtapose an alternative viewing of methodological problems in economic orthodoxy. Whereas my viewing of Positive economics focuses on the logic and structure of Positive economics as a coherent whole, Hollis and Nell's viewing focuses on, mainly, the logical defects of Positive economics per se and on a critique in terms of an alternative methodology. I consider my viewing if not more advantageous, then complementary to Hollis and Nell's, because it depicts the logical

and structural relations of Positive economics from within rather than from an alternative point of view.

Another reason for commenting on Hollis and Nell's critique is that the claims to objectivity made by them on behalf of their alternative are too strong. I shall base my criticism on the assumption, presented in the introductory note of chapter 7, that there is no theoretical system, either epistemological or ontological, that is entirely free of contradictions, and which provides the ultimate solution to the Inductive and Deductive Problems.

Obviously, it is neither intended nor pretended to offer a complete criticism of "Rational Economic Man" as this would form the basis for another thesis. What is suggested here are limited observations on a very small range of issues.

#### B. SCIENTIFIC MAN VERSUS ECONOMIC MAN

Most critics of Positive economics attack the assumption of the independence between theory and fact that helps Positive economists consolidate their claims to objectivity and reality. "We believe, in general," say Hollis and Nell, "that no clear or unique line can be drawn between 'fact' and 'interpretation'"<sup>3</sup>. Thus, most of the critics who follow this line of criticism turn to another system - encompassing an epistemological and an ontological theory - which substitutes for the positivist 'objectivity' another one (see chapter 6). For example Hollis and Nell claim that their "argument proceeds from the premiss that the world is the way it is"<sup>4</sup>. The

difference is that the world of Hollis and Nell is an axiomatic one in contradistinction to the positivist a posteriori one. "[T]he a priori ramifications of the concept of ability to reproduce", continue Hollis and Nell, "are, we believe, the root of production theory and so of any sound general economic theory"<sup>5</sup>. Once the independence of theory from facts is denied then necessarily one has to resort to another criterion of objectivity, if one has to act scientifically. Since Pragmatism is rejected<sup>6</sup>, Hollis and Nell turn to Rationalism. In other words because theories and facts are inseparable and because one cannot maintain an analytic and synthetic distinction, one has to use both theory and fact in order to select between alternative interpretations. Validation, therefore, is transferred from empirical facts to 'real definitions'.

'Real definitions' are neither empty analytical propositions nor strictly speaking empirical (measurable) facts. For example the definition of an economic system as one of a producing and self-reproducing unit<sup>7</sup>, is a definition that incorporates the 'essences'<sup>8</sup> of the process in both a factual and a logical way<sup>9</sup>. Factual because it depends upon self evident, conceptual, perceptions<sup>10</sup>, and logical because it draws the logical inter-dependencies and ramifications from these 'conceptual perceptions' in the style of Euclidean geometry. Furthermore, these "real definitions" are 'necessarily true'.

By a conceptually necessary truth we mean a necessary truth in which some constants which do not belong to formal logic occur essentially . . . we shall call them conceptual, in that they rely on basic concepts under which an activity must fall, if it is to be correctly identified as theoretically significant activity . . . explanation takes place when a conclusion is deduced from a sound and independent theory.<sup>11</sup>

These truths, therefore, are not empty, in the positivist sense of analytic truths, but they say something about the real world. They claim that the world must be as it is<sup>12</sup>. In contrast to Positivism which claims that the world could be otherwise, Rationalism claims that the world is as it is and could not be otherwise<sup>13</sup>.

As far as epistemology is concerned, Hollis and Nell reverse the positivist position from empirical facts validating theories to a priori conceptual truths validating empirical facts. As far as ontology is concerned they stipulate an economic world defined by production and reproduction (in their 'essential' and 'conceptual' sense). Here too they reverse the positivist position from subjective value theory to 'objective' production relations<sup>14</sup>.

However, in the fundamental structure of 'Rationalistic' economics an assumption is held which is also vital to Positive economics. Even though Hollis and Nell try to "entwine the Inductive and Deductive Problems"<sup>15</sup> in a synthesis which will produce a solution to the Problems of Knowledge, they do this at the cost of creating two 'realities': one which belongs to the scientist and one to the 'bearer' of economic variables.

The first assumption that Hollis and Nell make is to base their theory of economic reality on their theory of scientific knowledge. Rationalism, it is argued, offers a criterion, i.e. real definitions, upon which to construct complex theoretical systems. It helps them to capture the 'essences' of all historical periods,

i.e. production and reproduction, and apply them to each contingent period which may vary from one historical stage to the other. Slavery, feudalism and capitalism, for instance, are three particular manifestations of these ubiquitous 'essences'. "We submit then", say Hollis and Nell, "that, the real definition or basic concept upon which economics must rest is that of a system's ability to reproduce itself . . . The actual details of different social structure are facts independent of economic theory"<sup>16</sup>.

However, the "actual details", i.e. the mode of exchange, the particular economic behaviour, ideology, etc., in short the Marxian superstructure, are determined by the particular mode of production in a given historical period. If one analyses the contents of 'super-structure' one will find that science, or any theory of knowledge for that matter, is part of this surface structure which changes and is shaped accordingly as the mode of production changes. If, therefore, Hollis and Nell wished to remain consistently Marxists they would have to submit that the scientific man does not possess properties different from the lay economic man (at least as far as ideology is concerned) but belongs to the same sphere of influence: the prevailing mode of production. In fact it is doubtful whether they have a choice to do otherwise. If for their economics they borrow a Classical-Marxian schema, however modified by Neo-Ricardianism, they also have to stick to a Marxian theory of knowledge which is inseparable from a theory of economic reality. The historical-materialist dimension given in the concept of 'praxis' allows only the idea of science reflecting and being shaped by the mode of

production<sup>17</sup>. If, on the other hand, they inject Marxism with a bit of Leibnizian or Spinozian Rationalism, then they have to resort to an idealist theory of knowledge which would, prima facie, contradict their Marxist theory of reality. If they stand by the first choice, i.e. 'pure' Marxism, then their theory of knowledge must change from 'real definitions' and 'axiomatic necessary truths' containing omnipresent 'essences' to contingent theories of science that reflect and are determined by the particular historical period. If, however, they retain the rationalist position, then they must resort to some kind of mechanistic, harmonious universe, God (in a figurative sense<sup>18</sup>) and a solution to the theory of knowledge which contains and presupposes the Deductive Problem.

To recapitulate the argument so far: it is maintained that the contradiction in Hollis and Nell's claims is that the general action of economic agents is determined by the production and self-reproduction of the economic process<sup>19</sup>, which is universal, necessary and true of any historical period, while the specific action is determined by the specific mode of production prevailing at a point in time. However, the action of the scientific agent, general or specific, is not determined by any historical and contingent factor and is free of any ideologies reflecting the modes of production. The rational scientific man is always there observing the world as it changes from feudalism to capitalism to socialism, while being free and unhampered to define the 'essences'. Thus, while Rational Economic man changes from period to period, Rational Scientific man remains unchanged.

## C. THE DEDUCTIVE PROBLEM

Connected with the above contradiction is the question whether in fact Hollis and Nell have solved the Deductive Problem. Their claim is that they have solved the Inductive Problem with the help of deduction:

Our solution to the Inductive Problem will be, in general, that without assumptions about continuity in the world scientific knowledge is impossible, and, in particular, that a correlation is an instance of a law if there is a theoretical explanation of its significance.<sup>20</sup>

Since one cannot know whether empirical correlations will remain so in the future, one has to revert to an axiomatic and necessary truth which will necessitate the repeated occurrence of the events.

Following this, Hollis and Nell, however, acknowledge the existence of the Deductive Problem; namely they ask, "how necessary truths are to be distinguished from others"<sup>21</sup>? Their solution is that,

economic theories are to be judged partly by whether they are backed by a suitable scientific method which is itself backed by a sound theory of knowledge, which has an answer to the Inductive and Deductive Problems.<sup>22</sup>

Since the Inductive Problem is solved, once deduction is brought into the picture, and since the Deductive Problem is supposed to be solved by an appeal to a "sound theory of knowledge", i.e. Rationalism, it appears that the system is complete and the infinite regresses plaguing the assumptions of Positive economics, Empiricism, and Pragmatism

finally find an end. However, the only question that remains to be answered is: how sound is the epistemology of Rationalism?

As has already been mentioned Rationalism propounds the need for true axioms<sup>23</sup>, and more particularly the need for real definitions. These are "necessary truth(s) in which non-logical [i.e. factually significant] constants occur . . . essentially"<sup>24</sup>. Immediately, however, Hollis and Nell feel inclined to qualify their claims by saying that,

We see a need to be able to justify assumptions . . .  
Ex hypothesei this cannot be done by proof or evidence. So we are at an impasse, unless we take a theory of knowledge which admits the existence of self-evident necessary truths.<sup>25</sup> (my emphasis)

Obviously one is still left with the need to justify, firstly, the theory of knowledge and, secondly, the "self-evident necessary truths". Having said that neoclassical assumptions are disputable, and therefore not self-evident and that, "'real definitions' make a better basis than introspected postulates"<sup>26</sup>, Hollis and Nell argue that, "if economic theory can be axiomatized, then its axioms have to be self-evidently true"<sup>27</sup>. These self-evident truths are a priori necessities that are logically and not psychologically self-evident<sup>28</sup>. Furthermore they contain real definitions. So, it is maintained that necessary truths are logically and factually compelling. However, I think that the above presents two problems: in the one case it is about the justification of the theory of reality, and in the other of the theory of knowledge. The 'ugly' head of the Deductive Problems arises in both cases.

In the first case, although Hollis and Nell in their theory of reality admit that axioms can be disputable - they claim that this "does not cancel the search for those axioms which state the real definitions of the basic concept of economics"<sup>29</sup> - they nevertheless revert to concepts in their real definitions, such as 'essences', which define all economic systems. The question, however, is: what makes it 'compelling' to assign the status of 'essential' to one set of concepts rather than another? It may be self-evident that man has to feed himself or that every system is a production and a reproduction unit, but it is by no means self-evident that this set of essential concepts should be the one and only one upon which economics must rest. Other truths may also be logically self-evident. Rationality and scarcity, for instance, may very well be two alternative self-evident truths.

In fact, Hollis and Nell, here and as everywhere else, anticipated this objection and in pages 242-245 they try to justify their choice of production and reproduction as the primary premises for economics, by invoking the conceptual necessity of this assumption. This point is important and needs to be discussed because, as they themselves admit, the question,

Why should we claim a necessarily privileged status for 'production' or, more exactly, the 'reproduction' of the economic system? . . . is dangerous as it is apparently difficult. For if we cannot provide a satisfactory answer, our whole argument is in jeopardy.<sup>30</sup>

Thus, they must provide arguments that prove the necessity of 'reproduction' as a basic premise against other contentions such as

choice and exchange. (As a matter of fact they consider other candidates as well, e.g. money, market, labour and capital. However, I believe that for the purposes of the argument most of these candidates may be subsumed under the generic terms of scarcity-rationality and the implied exchange and choice.)

Thus, "The reproduction of the system . . . is primary", because other assumptions such as, "Choice depends upon choosers, exchange upon traders, labour upon workers, and so on. Choosers need reasons and abilities, traders must have goods and skills"<sup>31</sup>. Firstly, they dispense with 'choice' by placing it within a specific (contingent) social context. Once this is admitted 'choice' loses its abstract, general and essential form, attributed by the Neo-classical economists, and acquires a significance that is limited by the existence and perpetuation of the social context, i.e. production and reproduction<sup>32</sup>. They further dispense with the idea of choice of production methods by saying that production methods are not chosen but "that society as a whole is responsible for what exists in it"<sup>33</sup>, i.e. that controlled preference of production methods, at least for the present, does not exist. The point about Hollis and Nell's argument dispensing with 'choice' is that, as they themselves admit<sup>34</sup>, although reproduction involves both a conceptual and physical necessity, because "outputs require inputs"<sup>35</sup>, nevertheless one cannot exclude the logical possibility in the future of a conceptual necessity for 'choice' when, as they point out, "we may be able to control and determine the shape and form of what exists in society"<sup>36</sup>. In other words the chronological dimension of 'future' is important

as far as a descriptive (as opposed to a logical) proposition is concerned, i.e. a proposition containing empirical and not conceptual terms. However, it is unimportant as far as conceptual necessity (i.e. not involving time) is assumed for 'choice'. If this is so then one cannot claim conceptual priority for reproduction simply because it precedes 'choice' in time, i.e. reproduction is not rendered necessary because it comes before choice. After all it is difficult to discover the historical origin of things, and even more difficult to claim that the chronological origin of things define conceptual first principles. Therefore, once these arguments are taken into consideration the priority of reproduction as a necessary premise becomes limited.

However, although Hollis and Nell put choice as "the most obvious and widely canvassed alternative"<sup>37</sup>, I think that the neo-classical concepts of scarcity and rationality and therefore exchange, are more important contenders (or perhaps choice might be seen as an outcome of scarcity and rationality). They dispense with these assumptions by claiming that, "Exchange depends on having something to exchange, so must be placed in a context of production"<sup>38</sup>. The question is, however, that if exchange implies scarcity, then how can one be certain that reproduction is more primary and necessary than scarcity in defining the nature and scope of economic theory? If resources are assumed scarce and if wants are assumed insatiable, then exchange is necessary, whereas it is not obvious why, under these assumptions, reproduction should be primary to scarcity. If one

accepts these assumptions, then scarcity will exist irrespective of whether the bearers of these insatiable wants reproduce or do not reproduce themselves. (It will exist as a logical first principle. Obviously, exchange cannot presuppose reproduction as reproduction cannot presuppose exchange.) Thus, in these circumstances scarcity may be more primary than reproduction. On the other hand, one could argue that if the continuation or the reproduction of the system is assumed as necessary then scarcity will depend upon the depletion of resources necessary for this reproduction. Thus, in these circumstances reproduction may be more primary than scarcity. As far as rationality is concerned, it is argued that it is an assumption which is more or less contingent on the social context and it might not have existed prior to any social formation; for instance people might not have exchanged their surpluses in any rational, i.e. maximizing, manner but, according to Hollis and Nell, might have lived in self-sufficient units and with fixed allocatory rules<sup>39</sup>. But to argue that reproduction is logically prior to scarcity is almost the same as saying that the egg made the chicken and not the other way around. Moreover, though exchange needs reproducing traders, goods and skills, one might also argue that reproduction, in order to be realized, needs exchange. It seems that the economic process is more dialectical than deterministically necessary. Reproduction might determine scarcity but also scarcity might determine reproduction. It should be noted however that I do not justify the alternatives of choice and exchange as the more suitable or necessary ones, but am only pointing out the conceptual possibility of alternative assumptions for economics, and the difficulty of delineating first principles

either conceptually or historically. I do not believe that one assumption is more necessary than another, but their necessity and primacy is manifested in a dialectical way. Although in a dialectical sequence you need to have a thesis, nonetheless at a point in this sequence the synthesis becomes a new antithesis that takes the place of the thesis in the renewed sequence.

Thus, if we accept Hollis and Nell's epistemological definition of 'necessary truths', then a different alternative than the one they propose may be the dialectic between reproduction and exchange. Since it incorporates as one part reproduction it fulfils all the requirements for a 'necessary truth' put forward by Hollis and Nell<sup>40</sup>. It follows that if the above dialectical alternative is a 'necessary truth' then it must be incompatible with either exchange or reproduction (as independent units) since they also may be viewed as 'necessary truths'. That is to say if, according to Hollis and Nell, reproduction constitutes one 'necessary truth', and scarcity another logically incompatible with it, then an assumption incorporating the dialectic of the two constitutes a third possible 'necessary truth' incompatible with the other two (and this is true even if exchange is not considered a 'necessary truth' since it is sufficient for reproduction to be recognized as such). This dialectic, therefore, becomes the premise for an economic theory different than the ones based on either scarcity or reproduction. If this is allowed, then Hollis and Nell's position is indeed "in jeopardy". This follows from their own assumption that they "cannot allow the possibility of different fundamental concepts"<sup>41</sup>. The point is that a dialectical

alternative is conceptually and logically a possible alternative and fundamental 'necessary truth'.

However, Hollis and Nell are more interested in the way that these 'necessary truths' are reached. For example, Robbins' postulates about ends and means and scarcity are derived from introspecting about the nature of economic reality. They contain a synthetic element which, according to Hollis and Nell, forbids them to be used as laws or axioms from which necessary theorems are generated. In addition since they are "reasoned about" or they "are disputable" they "are open to more than one interpretation"<sup>42</sup> and therefore are not necessary. The sense with which Hollis and Nell want to use self-evident truths is epistemological rather than introspective. By epistemological they mean that 'necessary truths' are "'known without proof of evidence'. Here it matters not whether all mankind assents but whether it can be proved self-evident"<sup>43</sup>. Proof of self-evidence lies in accepting an epistemology which admits a priori truths. In choosing therefore a priori necessary truths they choose the epistemology of Rationalism. However, what makes it self-evident that Rationalism is necessarily true and self-evident? For instance Keynes argued that,

propositions in which our rational belief is both certain and direct, are said to be self-evident . . . But we must admit, I think, that this too is relative to the constitution of the human mind, and that the constitution of the human mind may vary in some degree from man to man. What is self-evident to me and what I really know, may be only a probable belief to you, or may form no part of your rational beliefs at all. And this may be true not only of such things as my existence, but of some logical axioms also.<sup>44</sup>

Furthermore, if one asks for the way by which the a priori truths are derived, Hollis and Nell would answer that "real definitions are reached only after 'close observation' and analysis"<sup>45</sup>. Surely, I hope, that by "close observation" they do not mean induction, because this would bring them against their own criticism. Also it cannot be claimed that "close observation and analysis" are the exclusive prerogatives of Rationalism. It could be hypothesized that with different a priori real definitions and "close observation and analysis" a different set of epistemologically self-evident concepts, other than production and reproduction, could be devised. What, then, are the criteria for choosing between alternative necessary truths about reality and knowledge? To provide an answer with a priori real definitions is simply to beg the question.

A different resort would be to argue that a sound theory rests on a sound epistemology which, according to Hollis and Nell, would solve the Inductive and Deductive Problems. It is true that the Inductive Problem was solved with deduction, but how was the Deductive Problem solved? "so we are at an impasse, unless we take a theory of knowledge which admits the existence of self-evident necessary truths"<sup>46</sup>. Thus, since Deduction cannot be solved with Induction one has to turn to another process of Deduction, namely Rationalism. However, it seems to be a contradiction to try to solve Deduction with Deduction. One always needs another Deduction which will solve the one Deduction before the last one<sup>47</sup>. Furthermore, what makes the rationalist theory of knowledge necessary? Certainly not real definitions about knowledge as this would create

an infinite regress. What then makes the acceptance of axiomatic truths compelling? Is it self-evident that Rationalism is the only epistemology good for economics? It may be necessary so far as a solution and a reaction to Positivism is concerned (in the same manner that Positivism used its epistemology to offer a solution to the Deductive Problem inherited by Scholasticism and Rationalism). However, as soon as Positivism is obliterated, then how is one to know that one knows correctly through Rationalism?

Finally, one cannot even compare Positive and Rational economics because even if Hollis and Nell say that the latter is more realistic<sup>48</sup>, nevertheless one does not have either Inductive nor Deductive criteria to determine 'realism'. So, without scope for choosing between alternative theories of knowledge, as Hollis and Nell admit, "economics as a scientific discipline is still somewhat hanging in the air"<sup>49</sup>.

## FOOTNOTES:

## APPENDIX:

1. I write this Appendix on Hollis and Nell's "Rational Economic Man", (op.cit.) after chapters 6 and 7 because of the use made of some of the ideas and concepts developed in chapter 7.
2. Hollis and Nell, "Rational Economic Man", op.cit., p.240.
3. Ibid., p.80.
4. Ibid., p.248.
5. Ibid., p.250.
6. Ibid., pp.152-169.
7. Ibid., p.250.
8. Ibid., p.178.
9. "Our strategy depends on being able to pick out what is conceptually essential and then to insist that what is essential is therefore to be found in practice", Hollis and Nell, ibid., p.254.
10. See M. Hollis, The Light of Reason: Rationalist Philosophers of the 17th Century, 1973, p.34.
11. Hollis and Nell, op.cit., p.172 and p.170. See also Hollis, ibid., p.19. It is difficult to see, however, what Hollis and Nell mean by, "independent theory", when already they have maintained that theories and facts are interrelated in order to criticize Empiricism. It seems illegitimate, therefore, to claim independence of theory when they admit that theory is dependent on fact and vice versa.
12. Hollis, ibid., p.20.
13. Ibid., p.20. See also Lazerowitz, "Philosophy and Illusion", op.cit., p.30.
14. A similar diagram as in Figure VIII could be drawn for Rationalism. As the separation between epistemology and ontology is maintained - in fact it is indispensable if one wants to act scientifically and seek knowledge - so is the relation between them. However, this relation between the opposition is transposed: the rationalist ontology is the one which is now necessary, while the epistemology is contingent. The ontology is necessary because, as we saw, it 'must be so'. The epistemology is contingent

(cont.)

## FOOTNOTES (cont.)

## APPENDIX:

14. (cont.) because it is conditional upon the concepts used. According to Hollis and Nell, "The Rationalism we propose is conceptual and the necessary truths it relies on are conditional" although "these are genuine necessities involving real definitions and true axioms", (op.cit., p.195 also p.172). The contingency in Rationalism is found in the conditional form knowledge takes. Although a conceptual definition contains a necessary truth, it also depends on a set of concepts that are contingent and form the basis for the necessary truth. A different set of concepts would produce a different necessary truth. Accordingly, Rationalism seems to be a reaction and a reversal of Positivism, solving the problems and contradictions inherent in it and creating its opposites. (For a discussion of necessity and contingency in Rationalism see Lazerowitz, ibid., pp.28-30.)
15. Hollis and Nell, ibid., p.195.
16. Ibid., p.250.
17. However, if this definition of 'praxis' is accepted, then even Marxism falls within a contradiction as soon as it attempts to postulate a ubiquitous concept of 'praxis' or a necessary truth of science which transcends the contingent boundaries of modes of production. According to Marxism most of the ideological systems, including science - and especially social science - reflect the prevailing mode of production. Thus, although Marxism explains all other ideological systems, it fails to explain itself and that it belongs to no mode of production while remaining aloof from any such contingent dependency.
18. Hollis, op.cit., p.9.
19. Hollis and Nell, ibid., p.190.
20. Ibid., p.13.
21. Ibid., p.12.
22. Ibid., p.13.
23. "By a 'sound theory' we mean a set of true axioms, including real definitions and their logical consequences", Hollis and Nell, ibid., p.181.
24. Ibid., p.172
25. Ibid., p.181.
26. Ibid., p.204.
27. Ibid., p.204.

## FOOTNOTES (cont.)

## APPENDIX:

28. Hollis and Nell, ibid., p.181.
29. Ibid., p.204.
30. Ibid., p.242
31. Ibid., p.243.
32. See ibid., pp.243-244.
33. Ibid., p.244
34. Ibid., p.245.
35. Ibid., p.245.
36. Ibid., p.244.
37. Ibid., p.243.
38. Ibid., p.244.
39. Ibid., p.244.
40. See ibid., pp.172-175.
41. Ibid., p.243.
42. Ibid., p.204.
43. Ibid., p.204.
44. Keynes, "Treatise on Probability", op.cit., p.18.
45. Hollis and Nell, ibid., p.204.
46. Ibid., p.181.
47. As Winch says, "a form of human activity can never be summed up in a set of explicit precepts. The activity 'goes beyond' the precepts. For instance, the precepts have to be applied in practice and, although we may formulate another, higher-order, set of precepts prescribing how the first set is to be applied, we cannot go further along this road without finding ourselves on the slippery road pointed out by Lewis Carroll in his paper . . . 'What the Tortoise said to Achilles'", "The Idea of a Social Science", op.cit., p.55.
48. Hollis and Nell, ibid., p.233.
49. Ibid., p.204.

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## CHAPTER 3:

ECONOMIC POLICY AND THE DEVELOPMENT  
OF THE CONTROVERSY

## A. POST-WAR MACROECONOMIC POLICY

Although the Monetary controversy dates back to Ricardo and the Bullionists and passes through the Currency and Banking schools conflict in the 19th century and through the Economic Depression, its growth takes enormous proportions during the period after the Second World War. This period, namely from 1950 to 1970, offers two types of evidence one of which was not available to the older disputes. The first one concerns the outcome of various policies, the success or failure of which were taken to justify the truth or falsity of the theories behind the policies - common to old and new discussions - and the second one concerns the accumulation of 'hard' empirical evidence, i.e. data produced through econometric analysis. This latter development, prima facie at least, seems to have been a decisive turning point in the scientific effort to find criteria of choice between the alternative hypotheses. However, it became eventually apparent that the power of this new type of evidence was not proved to be as decisive as it was thought it would be. The controversy, despite the accumulation of empirical evidence from both sides, still remains unresolved.

One might argue, however, that the Monetarist controversy cannot be properly called persistent, since efforts to resolve it have started only recently. It is only 15 to 20 years that there has been continuous empirical assessment of the conflicting theories (though strictly speaking empirical testing started with the use of the Oxford Institute questionnaire evidence on business investment behaviour, by the Radcliffe Committee). It follows, therefore, from this argument that given time the development of new tests and empirical techniques will lead to the eventual resolution of the controversy. Nevertheless, this argument applies only to the post-war controversy and relates only to the particular empirical form associated with the Monetarists and Fiscalists. It disregards the fact that the controversy is not new. That it goes back to Hume and Ricardo, and passing through the Banking and Currency Schools, it reaches Keynes, Robertson, and Pigou, up to the Macmillan and Radcliffe Committees. Although the form of the debate has changed, and now

there is massive empirical evidence, as well as theoretical sophistication of the issues, the result remains the same: there are always two views concerning the role of money. And neither the ad hoc judgements used by earlier economists, nor the more 'scientific' empirical evidence used now, contributed much towards a definite choice between the alternative views. Of course nobody knows whether the controversy will go on persisting. The point, however, is that since the emergence of the Oxford Institute survey evidence of the interest-elasticity of business expenditures, empirical testing has been shown inadequate to resolve the debate. The hope that it will resolve it, therefore, constitutes only a judgement of faith and neither a logical nor an empirical argument. One might argue, however, that the monetarist view is more acceptable now. Although it is granted that the Monetarists have gained some ground in the debate, this does not mean, from what seems in the empirical literature, that their tests are more conclusive. This is not true according to Keynesians (see F Modigliani "The Monetarist Controversy, Or Should We Forsake Stabilization Policies", American Economic Review, 1977). This can be ascribed more to inflation, stagflation and the inadequacy of demand management policies, in the face of adverse conditions. Though, for instance, money now is taken more seriously (something that was undisputed by sophisticated Keynesians - see Chapter 5 below), academic economists still persist in their conflicting views, and still produce a great deal of empirical evidence to support either the relative importance of monetary or of fiscal policy (see e.g. J L Stein (ed), Monetarism, Amsterdam, 1976, B Friedman "Even the St Louis Model Now Believes in Fiscal Policy", Journal of Money, Credit and Banking, May 1977, and K Carlson, "Does the St Louis Equation Now Believe in Fiscal Policy?", FRB of St Louis Review, February 1978). In my opinion, the debate about money persists because it concerns the fundamental assumptions of economics. It concerns the nature of the economy, i.e. the flexibility and stability of the market mechanism, and the role of money in it (neutrality of money, money as 'veil', etc). Different views are involved about the role of uncertainty, perfect knowledge of economic agents, the price system and the optimization process. These same views that were at issue during the debate between Keynes and the 'classics', have re-emerged and have been incorporated (in a more quantitative and empirical form) in the views of Fiscalists and Monetarists. The former view the economy as inherently unstable

and intervene to stabilize it, the latter view the economy as sufficiently stable to guarantee the proper functioning of the 'free' market. The conflict thus persists despite the qualitative and quantitative evidence, because methodological and ideological positions are continuously used to interpret and qualify the evidence. Alternative interpretations of the ambiguous definitions of theory and fact within Positive economics allow such interpretations. Furthermore, as we shall see, these ambiguities are built into the conceptual structure of Positive economics in such a way as to permit economists in using, on the one hand, empirical testing as the only criterion of choice, and on the other, in persistently interpreting it to suit their particular ideological positions.

For the moment I shall leave this latter type of evidence aside until chapter 5 and first give a rough account of the development of monetary and fiscal policy in the period from 1950 to 1970, as this will set out the historical perspective and context through which to look at the controversy. The choice of this period is significant because it contains the most important turning points that account mainly for the apparent failure of Keynesian policy and for the consequent 'counter revolution' of Monetarism<sup>3</sup> partly brought about by the success of monetary policy<sup>4</sup>. The increase in the intensity of the controversy is tied up to the rise of Monetarism as a separate school, according to its new version given by the Chicago school and Friedman. Monetarism became more apparent with the economic events around the middle and late '60s especially in the United States. I have put the emphasis on events in the American economic scene because, firstly, it is the country where Monetarism grew and intensified the controversy, and secondly, because it is the country where the greatest part of the debate literature is being written.

In accounting for the chronological development of the controversy I separate the post-war period into two sub-periods, firstly from 1950 to 1960, and secondly from 1960 to 1970.

#### a. 1950-1960

This particular sub-period is considered to be less important than the one following it; yet it is significant because it saw the emergence of an independent Federal Reserve System in the United States, reflecting the need for an official implementation of Keynesian policy,

and because it included the peak of the rise of Keynesian<sup>5</sup> macroeconomic thought and policy, the "high-tide" of Keynesianism as Johnson puts it<sup>6</sup>. This peak has its causes in the apparent<sup>7</sup> success Fiscalism enjoyed during the interwar period. Before the Great Crash Monetarism was considered a very potent and effective theory, and ". . . with respect to the early 1930's, it can be said that the United States in the past has relied in large part on monetary policy as its major instrument for achieving price stability and high employment"<sup>8</sup>.

However, the climate of the aftermath of the Depression, i.e. the interpretation of the ineffectiveness of monetary theory due to very low levels of interest rate, and the publication of Keynes' "General Theory" in 1936, brought about the collapse of the Quantity Theory and the emergence of an era of macroeconomic thinking based upon the Keynesian explanation of economic reality. This was accentuated by the fact that national economies immediately after the Second World War enjoyed a relatively high economic growth believed to be attributed to the application of Keynesian theory<sup>9</sup>. However, underlying inflationary tendencies, and some 'voices' crying in the 'wilderness of Keynesianism' (mainly Friedman's<sup>10</sup>) becoming eventually heard, started to discredit the Keynesian thesis.

The mark of this period was the publication of the Radcliffe Report in 1959, which reflected both the peak and the trough of Keynesianism. The peak because it offered an extreme - and unsophisticated - Keynesian policy disregarding the control of changes in the money supply, and the trough because of the immense criticism that it received for its extremity. In fact, due to the overdue neglect of monetary policy, its proposals - at least for the academic economists - were relatively neglected. It was alleged that its propositions were contradicted by the empirical facts<sup>11</sup>.

## FOOTNOTES (cont.)

## PART III: CHAPTER 6:

## 64. Continued from Page 287.

Recognition should also be given to the claim that the controversy is due to the critical nature of the scientific process. There is never a decisive test that forms an absolute criterion of choice between theories. Every test is falsifiable, since it is empirical, and the critical spirit of the scientist sees to it. There is no doubt that this factor is important in explaining the persistence of controversy, since all issues and their tests are open to continuous falsification. However, in accepting this explanation - or others similar to it, such as that empirical techniques are ineffective - as sufficient and adequate, we fail to recognize all the other elements involved in scientific controversies. We fail to depict the particular ideas and their interrelations that form the conceptual 'matrix' of conflict. The fallibilist nature of science perhaps explains the existence of controversies, but it does not tell us anything about the ideological and methodological elements involved, nor about the logical ambiguities stemming from them. By delineating the relations of these ideas the structure and the logic, rather than only the existence, of the controversy is explained. Such an explanation may tell us something about the persistent theoretical and methodological qualifications that are used to interpret empirical criteria of choice, as well as about the systematic relations between particular methodologies and ontologies that back such qualifications.

## 65. Addition.

For a discussion of the so-called 'Growth of Knowledge' theories of science, see the introduction of A A Arouh "The logic of conflict in the appraisal of economic theories" forthcoming.

This explanation differs from a causal explanation, which attributes persistent controversy to only ideological factors, or to socio-historical forces, or to other external causes, in that it traces out the logical relations that connect the conceptual structure used by the controversialists from an internal point of view. The conflict will not be attributed to only external causes (although these may also be important), but it will be reflected in the ambiguous nature of this conceptual structure, and the logical tension stemming from it. Accordingly, Positive economics will be taken as a system of beliefs, a myth (not in the pejorative sense), the analysis of which will reveal the logic that nurtures the Monetarist and F-Twist controversies (for such analyses of myths see E Leach, Genesis as Myth, 1969 and C Levi-Strauss "Mythologiques" in L'Homme Nu, Paris, 1971 and E Leach (ed) The Structural Study of Myth and Totemism, London, 1967). This analysis has an advantage over uni-dimensional causal explanations, in that it fits the binary nature of the phenomenon in question. The methodological and ideological conflict involved will be analysed in terms of binary oppositions that are already built into the dichotomies of Positive economics. This will constitute a structural explanation of conflict in Positive economics, which will delineate, first, the methodological and ideological reasons that are used to qualify the conflicting positions in the face of contradicting evidence and, second, the logical relations that stem from the distinctions of Positive economics to allow divergent interpretations of the agreed upon criterion of positivist objectivity, i.e. empirical testing. The explanation is called structural because it attempts to reveal the deep structure of controversy in economics, in terms of which both the conflict and the unity may be understood. This deep structure takes the form of structural relations. Structural relations in my use of the expression are binary oppositions, and in the case in question these binary oppositions are implied in the distinctions of Positive economics. They are logically related to form a unity or identity. Thus each distinction contains a structural relation. The logical tension between conflict and unity implied in these oppositions is the key to the understanding the conflicting interpretations of Positive economics.

At the contextual (historical) level certain developments in both Positivism and Neoclassical economics facilitated the marriage of the two. Firstly, both Positivism and Neoclassical economics reject anything that has to do with metaphysics<sup>29</sup>. 'Essences' or 'wholes' are considered pseudoproblems<sup>30</sup>, and their study should be left to art rather than science<sup>31</sup>. In consequence, Positivism is led to develop a theory of knowledge based on methodological individualism and Neoclassical economics a theory of reality on economic individualism. The compartmentalization of reality into discrete units renders scientific observation feasible. The actions of individuals are cast in terms of rational choices or preferences the classification of which enables the application of measurement techniques<sup>32</sup>. By divesting reality from notions such as 'whole' or 'essence' Positivism opens the way for experimentation and science<sup>33</sup>.

Another development that brought the alliance<sup>34</sup> closer together - also related to the 'anti-metaphysics' campaign - is the distinction between analytic and synthetic, on the one hand, and means and ends on the other. Although Positivists acknowledge the existence of metaphysical statements they render them harmless by ascribing to them a vacuous role. Such statements, it is believed, are empty because they do not correspond to any part of the factual domain. Some of them are also tautologies since their meaning depends on the parts from which they are constructed. (A distinction should be drawn, however, between metaphysical and analytic statements. Metaphysical statements of the form 'for every x there is somewhere a y' can be characterized as vacuous because there is no empirical statement that can verify or falsify it. In this sense their vacuity consists in the undecidability of their truth-value. On the other hand, analytic statements may be called vacuous because they imply a tautology in the sense that tautologies do not describe or say, but only show, and so are true whatever the empirical facts may be). The function of these statements should, therefore, remain heuristic and their analysis should be referred to the linguist rather than to the scientist. In contrast synthetic statements are about facts derived from the real